FUSION ENERGY WORKSHOP

JOINT HEARING

BEFORE THE

SUBCOMMITTEE ON
ENERGY RESEARCH AND PRODUCTION

OF THE

COMMITTEE ON
SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

AND THE

SUBCOMMITTEE ON
ENERGY RESEARCH AND DEVELOPMENT

OF THE

COMMITTEE ON
ENERGY AND NATURAL RESOURCES
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FUSION ENERGY WORKSHOP

WEDNESDAY, SEPTEMBER 8, 1982

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION,
U.S. SENATE,
COMMITTEE ON ENERGY AND NATURAL RESOURCES,
SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT,
Washington, D.C.

The committees met, pursuant to notice, at 9 a.m., in room 2318, Rayburn House Office Building, John M. Richardson (senior staff officer, National Academy of Sciences) presiding.

Members present: Senator Domenici and Representative Bouquard.

Mr. RICHARDSON. First, I would like to tell you who I am and then introduce our guests. I am John Richardson, on the staff of the National Academy of Sciences. I am by no means an expert in fusion energy, but I am very honored to chair this workshop. I would now like to introduce Senator Pete Domenici, chairman of the Subcommittee on Energy Research and Development, who is prepared to welcome you and make opening remarks.

Senator?

Senator DOMENICI. Thank you very much, Dr. Richardson.

First I want to welcome and thank the participants from a number of distinguished institutions and organizations in this country and from abroad. If you are not an expert, Doctor, I don't know where that puts me, but I am genuinely interested in the energy field and certainly more specifically in fusion.

I think we all know that Congress approved an aggressive fusion research bill, the Magnetic Fusion Energy Act, during the 96th Congress. The administration has proposed a program which is a step up from past activity, but is not as aggressive as many would like. I appreciate and understand the administration's need to balance an aggressive program with its overall efforts for fiscal restraint.

In its more conservative approach to the construction of a device for research into engineering problems, the Department claimed that a large investment in a particular device would be premature; instead a smaller and more flexible device would be sought while scientific questions about what type device to pursue in the future were investigated on existing machines and ones under construction.

While I tend to support that approach, obviously we are here today to learn more from the Department and more from the distinguished experts from the United States and the world on that approach and what it might mean for the future.

I am here to again welcome and thank you. I think you know that a number of staff people were instrumental in advising us and keeping us posted on where we are and where we are going.

With that, I want to thank you again, in particular you, Dr. Richardson, to agree to moderate the workshop.

Mr. RICHARDSON. Thank you, Senator, for starting us off on the right foot.

We would now like to hear from Jack Dugan, who is staff director for Representative Marilyn Bouquard, who I understand is unable to be here, but will be represented by Jack.

STATEMENT OF DR. JACK DUGAN, STAFF DIRECTOR FOR REPRESENTATIVE MARILYN LLOYD BOUQUARD

Dr. DUGAN. Thank you, John.

Mrs. Bouquard would also like to thank the participants for taking the time to be involved in the workshop. She would also like to thank Senator Domenici for working with us to put this together and she looks forward to future increased interaction with the people on the other side of the Hill.

The chairman also sees the workshop as a natural extension of the advisory panel which the subcommittee has had for several years now and has offered valuable advice. She hopes that the subcommittee and the committee can capitalize on what we hear today and on the basis of any new directions. Our committee is very concerned about maintaining some significant industry involvement in the program under what appears to be a flat-funding future objective for the program.

Thank you again.

Mr. RICHARDSON. Thank you, Jack.

I think maybe I will make one or two points before proceeding to our first speaker.

I think that this workshop today is bound to be something of a mixture of science on the one hand and technology and public policy on the other. We have a very serious issue of technology and public policy with respect to fusion, but that issue cannot really be addressed without a sound understanding of the underlying science. So although technology and public policy is the lead topic this morning, I would caution you to be prepared for the scientific and engineering points that will be made.

About 3 weeks ago, as you will hear later in the morning, the Academy of Sciences, under the sponsorship of the Department of Energy, conducted a similar workshop and a number of interesting points were made at that workshop. The most interesting one was this: that in this technology we do have something totally new. What we have is a new state matter, not the familiar solid or gas or liquid. We have temperatures not attained before. We have the possiblity of containing physical material without recourse to mechanical boundaries such as the sides of a bottle. All of this is new technology, yet this new technology is proposed only to generate

electricity a little more cheaply and a little better than our existing methods.

The point that was brought out was that possibly this technology contains something better than that. As you listen to the speakers this morning, I would like you to bear in mind that the technology of magnetic fusion takes us a long way beyond technologies that are familiar to us and may have applications that go beyond those that are foreseen today.

With that single point, I would now like to introduce John Clarke, who is presently the Associate Director for Fusion of the Office of Energy Research, U.S. Department of Energy. In this position, John has responsibility for the fusion program of the Department of Energy. He has spent almost his entire career in the field of fusion having started as a graduate student at MIT and having received his doctorate of nuclear engineering from MIT. He formerly was the Director of the Oak Ridge National Laboratory fusion program. John is well known to all of you in the fusion community, but I make those biographical remarks to introduce him to those of you who may not be intimate members of that community.

John?

STATEMENT OF DR. JOHN CLARKE, ASSOCIATE DIRECTOR, OFFICE OF FUSION ENERGY, U.S. DEPARTMENT OF ENERGY

Dr. CLARKE. Thank you, John. When somebody introduces me by saying I am well-known, the word notorious always jumps to mind. I hope that is not the case.

I am going to be introducing the topic of fusion today, so I will start with some of the basic reasons for the fusion program and some of the basic science involved with fusion and then proceed to a discussion of our strategic approach to fusion in the Department of Energy.

FUSION —

A PRINCIPAL ENERGY OPTION FOR THE UNITED STATES EARLY IN THE NEXT CENTURY

- **FUEL RESERVES ARE VAST, WIDESPREAD, AND AVAILABLE**
- **ENVIRONMENTAL AND SAFETY ASPECTS APPEAR REASONABLE AND ACCEPTABLE**
- **MULTIPLE END-USE APPLICATIONS POSSIBLE, INCLUDING CENTRAL STATION POWER AND SYNTHETIC/FISSILE FUELS**
- **PROGRESS TOWARD PRACTICAL CAPABILITY HAS BEEN CONTINUOUS AND SIGNIFICANT TO POINT OF BELIEF IN SCIENTIFIC FEASIBILITY**

SLIDE 1

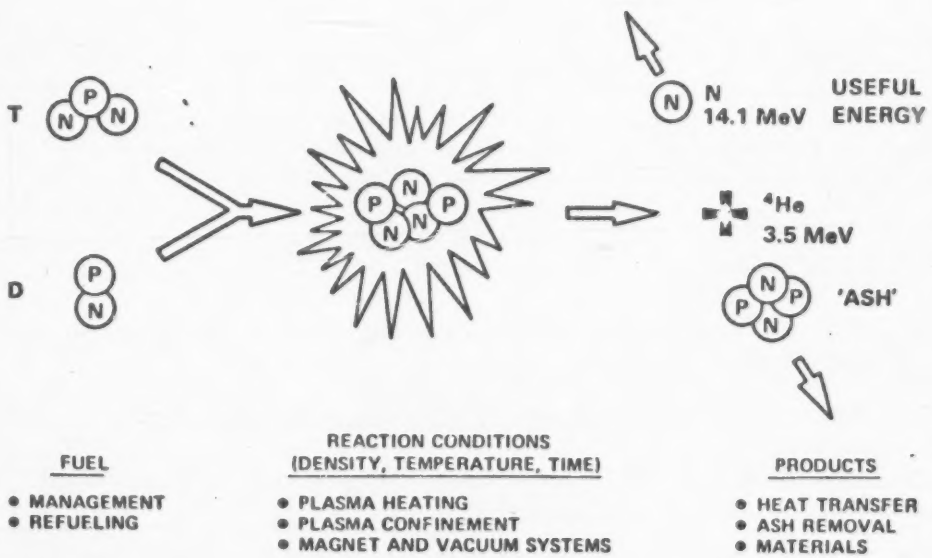
The administration recognizes that fusion is a principal energy option for the United States in the next century. It recognizes the

importance of this program. The reasons for the importance are indicated on the slide (slide 1). Fuel reserves are vast, widespread and available not only to the United States, but the world. Therefore, there are fundamental international implications with respect to the fusion program. The environmental and safety aspects appear reasonable from all the work we have done so far. Consequently, we feel that this source of energy can be developed into something that would be useful with minimal impact on the public.

It also has the advantage that there are multiple end-use applications. Fusion is a very open-ended technology. In principle, it can be used for central station power, but it could be used for producting fissile material to fuel fission reactors.

Finally, and most importantly, it seems that it is achievable in the sense that we have continuous progress for the last dozen years and all of the results that have been obtained recently give us great confidence that the program will continue to be successful.

THE FUSION PROCESS



SLIDE 2

The next slide (slide 2) indicates the basic fusion process. The reaction indicated is that of tritium and deuterium, which when combined will fuse into a helium 5 nucleus which decays into a energetic neutron and alpha particle. This reaction illustrates simultaneously much to the technology and scientific problems that is associated with fusion.

In the first place, tritium, the basic reactant in the process, is radioactive. Therefore, a fusion powerplant will have to deal with the problem of containing and handling this radioactive gas safely. Of course, the amount of this gas is very, very small, but nonetheless this is something that has to be taken into account. So in our program, we have worked on fuel management as well as refueling using tritium.

The reaction won't take place except at very high temperatures and it also has to take place at a sufficient high density to produce useful power. In order to create those conditions on earth, we have to understand the process of plasma heating, plasma confinement, and we have to develop the technology of magnets to hold this plasma together while it is reacting. We also have to deal with the problems of vacuum systems. The vacuum systems may seem to be a mundane problem, but this is a difficulty that held up the progress in the field for nearly 20 years.

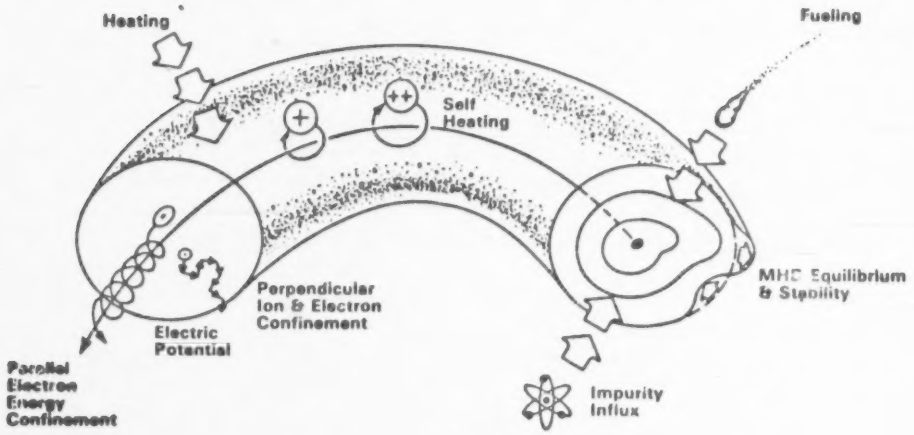
Finally, once we produce the conditions for fusion reactions, we have to deal with the products. The 14 MeV-neutron produced is an extremely valuable product since, because of its energy, it can be used to produce fissile fuel. However, also because of its energy, it produces different and unusual materials damage in the structure surrounding the reactor.

The alpha particle ash is a helium nucleus, but contains enough energy after the reaction to be useful for heating the plasma. So for an efficient fusion system we have to understand how to contain the alpha particle reaction product energy and use it to replace the external heating. We also have to understand cold ash removal from the plasma. And we have to understand question of materials damage produced at the plasma edge.

We have recently discovered this D–T reaction is not as simple as most of us had believed. Recent calculations has led to the belief that the magnitude of the fusion reaction can be increased by approximately 50 percent if we can polarize the deuterium and tritium nuclei that are the ingredients of the reaction.

If we are able to do this—and I say if because it is at this point a very theoretical prediction and has not yet been subject to experimental test, we may also be able to control the direction of emission of the neutron and the alpha particle which gives us increased flexibility in designing an effective fusion reactor. I think it is a very significant discovery as much for the fact that it indicates the open-endedness of fusion. We are making discoveries in all aspects of this field, including the basic reaction itself.

THE EIGHT FUSION PHYSICS ISSUES MUST BE ADDRESSED AS A COMPLETE PLASMA SYSTEM



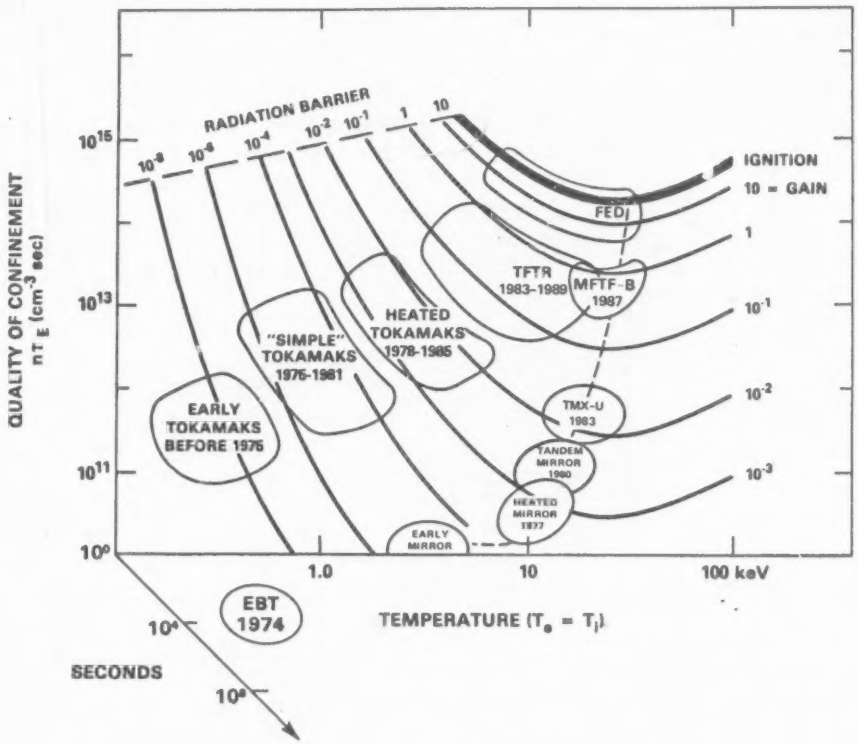
SLIDE 3

The next slide (slide 3) is a cartoon but it sums up the basic problems we have to address in fusion. What is shown is a piece of a fusion plasma, the cartoon indicates that we have to solve the problem of injecting fuel into that plasma to produce enough plasma density to react. We have to control the influx of nonhydrogenic impurity atoms to prevent them from radiating away the energy we produce. We have to inject enough heat into the plasma to bring it up to reaction temperature. If we are successful in fueling it and heating it, we simultaneously succeed in raising the pressure to the point where the pressure of the plasma begins pushing on the magnetic field and we have to solve the problem of magnetohydrodynamic stability of the field that holds the plasma together.

We have to understand and be able to handle the loss of energy from the system. If the energy leaks out too fast, we can heat it, of course, but we won't be able to release enough energy from the reaction to compensate for the heat we have to put in. This is one of the fundamental mysteries with which we are dealing. We are making progress in all of these aspects of containing plasma, but as yet we do not understand the basic mechanisms for the loss of energy from the plasma. In spite of this lack of fundamental understanding, our experiments have shown us that we should be able to get sufficient energy confinement, to create a self-sustaining fusion reaction.

The results at the recent International Atomic Energy Agency meeting in Baltimore this last week indicate that in terms of energy confinement and plasma pressure, we should be able to produce an ignited fusion plasma. So we making progress. But I want to emphasize that the scientific understanding of fusion is not yet

complete and we have a great deal of fundamental work to do in order to complete that understanding. Stated another way, we can put together what we know today about fusion plasma and create a power producing plasma, but our knowledge is by no means complete enough to optimize that power production for practical uses at this point.



SLIDE 4

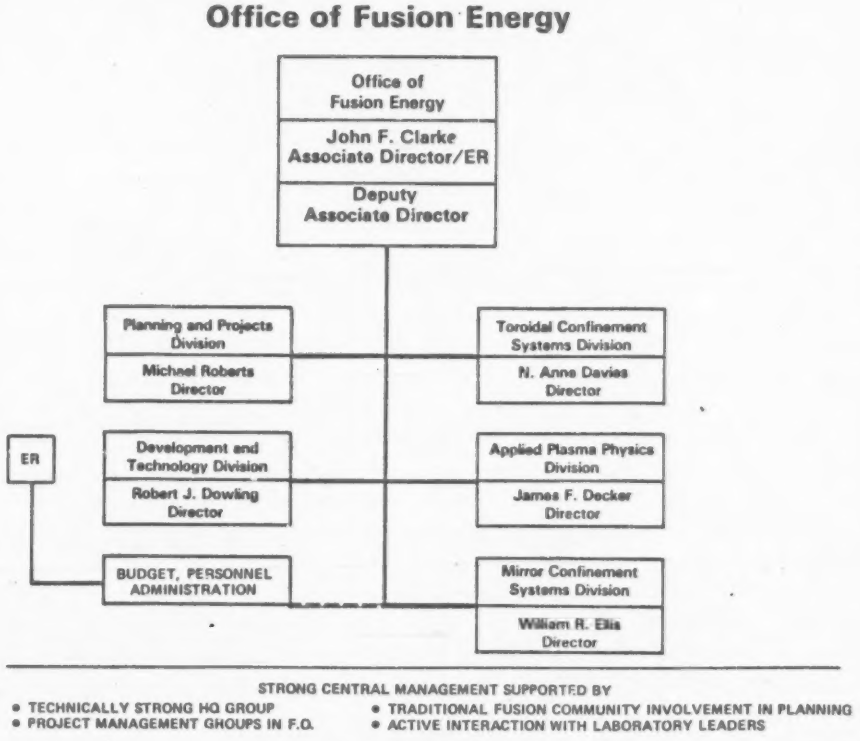
The next slide (slide 4) indicates the kind of progress that we have had in fusion. There is a central trend on this graph, the graph shows the quality of confinement which is a gross measure of the ability to contain energy in fusion plasma versus the temperature of that plasma. We have to reach the upper right-hand corner. The main thrust of the world's fusion program is shown along the center of that graph, the Tokamak line. It is in the Tokamak that we have produced most of our high temperature and high pressure reactions.

In the United States we are following simultaneously a parallel line of mirror development. Due to a fundamental advance in the understanding of mirror combined plasma in about 1978, the United States has begun to develop something called the tandem mirror fusion reactor. This is a magnetic confinement concept which is fundamentally different from a Tokamak, it is an open-ended magnetic structure as opposed to closed or toroidal structure. That difference leads to fundamental differences in the physical

problems of these two systems and will lead to reactors with different characteristics.

The United States is pursuing both of these lines and we are also pursuing a number of alternative confinement systems, one of which is illustrated on the graph on a perpendicular axis that indicates the length of time the experiment is operated. In the interest of economy, both the Tokamak and mirror systems are pulsed devices at the present time; but the EBT, which is a small device operating at Oak Ridge, is a device that operates in a steady state which gives us some confidence that indeed we will eventually be able to operate all of these fusion systems in steady state.

The purpose of these alternate confinement devices is to open up new areas of science, new characteristic operating regimes of fusion reactors, as in the case of EBT which has been exploring among other things the steady state aspects. The alternates give us a lot of insight into the basic physical processes that we have to understand in order to optimize and make the main line concepts into acceptable reactors.


Office of Fusion Energy
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Deputy Associate Director
Planning and Projects Division
Michael Roberts Director
Toroidal Confinement Systems Division
N. Anne Davies Director
ER
Development and Technology Division
Robert J. Dowling Director
Applied Plasma Physics Division
James F. Decker Director
BUDGET, PERSONNEL ADMINISTRATION
Mirror Confinement Systems Division
William R. Ellis Director
STRONG CENTRAL MANAGEMENT SUPPORTED BY
• TECHNICALLY STRONG HQ GROUP
• PROJECT MANAGEMENT GROUPS IN F.O.
• TRADITIONAL FUSION COMMUNITY INVOLVEMENT IN PLANNING
• ACTIVE INTERACTION WITH LABORATORY LEADERS

Office of Fusion Energy

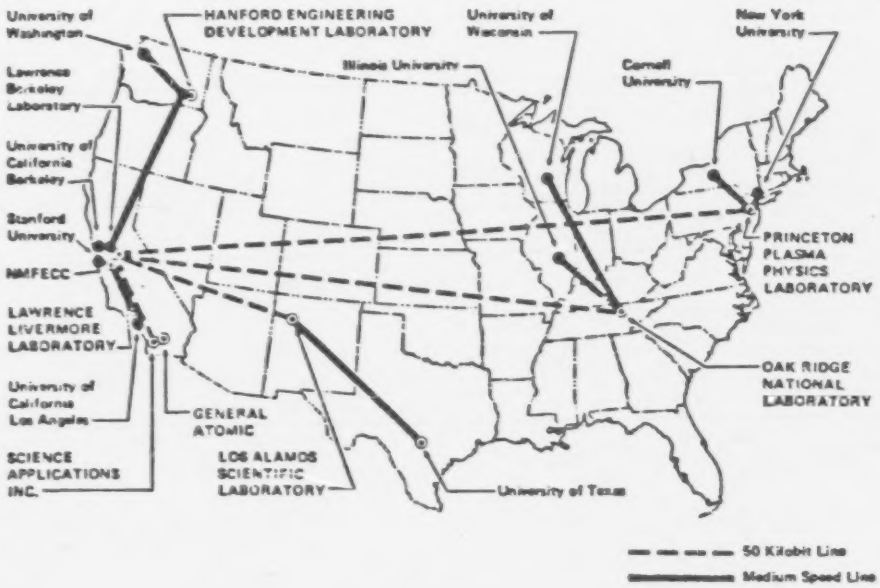
SLIDE 5

This is a complicated program and the next slide (slide 5) indicates the way we manage it. As John indicated, I am Associate Director of the Office of Energy Research and I also serve as Director of the Office of Fusion Energy. In this office we have a number of divisions. This is the Tokamak program headed by Dr. Davies. This is the mirror program run by Dr. Ellis. We also have a division of

development and technology because we are simultaneously developing the technology required for fusion reactors under the direction of Dr. Robert Dowling. We have a planning and projects division handling international affairs under Dr. Michael Roberts. That is an office which is probably the most active in the Office of Fusion Research in recent months because of the increasing involvement with the fusion programs of other nations.

Traditionally, the fusion program has had strong central management with a strong headquarters group and we have a well-planned program, as a result of a planning process which involves the leaders of the magnetic fusion program.

MAJOR COMPONENTS OF THE NATIONAL MAGNETIC FUSION ENERGY COMPUTER NETWORK



SLIDE 6

The next slide, (slide 6) illustrates the breadth of the fusion program. The laboratories involved span the United States. Shown here is the computer system which ties together all those laboratories into a unified program. The computer system we have, is not only a useful tool for exchanging information and doing the large computations required in fusion, but it is also symbolic of the integrated nature of the magnetic fusion program.

THIS ADMINISTRATION HAS SET OUT BOUNDARY CONDITIONS FOR THE FUSION PROGRAM

- **Critical Long Term R&D Programs Are a Federal Responsibility**
- **We Should Demonstrate the Scientific and Engineering *Feasibility* of Fusion**
- **We Should Be Prepared to Make Difficult *Choices***
- **We Should Plan on Relatively *Constant* Budgets for '84-'88**

As a Result, We Must Focus on Feasibility Issues, Progressing But Staying Within Fixed Budgets By Making the Difficult Choices.

SLIDE 7

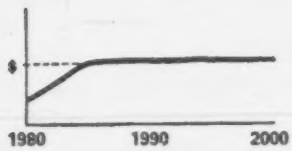
Let me come to the question of the administration's position on fusion (slide 7). As I indicated, the administration feels that the fusion program is a Federal responsiblity. It is fundamentally important for the long range future of the country and because of that long range, it is a Federal responsibility. Specifically the administration feels that it is a Federal responsibility to demonstrate the scientific and energy feasibility of fusion. However, because of the constraints on Federal budget, we must be prepared to carry out the fusion program in a cost-effective and economic fashion. That means we will have to make what are laughingly referred to as difficult choices. In practice this means that in order to make progress, we will have to complete projects, turn them off and replace them with the advanced facilities needed to go forward.

We must plan on relatively constant budgets for the 1984–88 period. This means that we will have to very carefully look at the activities within the fusion program and choose those activities which will allow us to make progress toward our goal within that budget constraint.

Our approach to these boundary conditions is to design a program that focuses on critical feasibility issues. That means that we will not necessarily take every aspect of plasma physics or technology and push it to the ultimate development required for commercial power. But we will carry out research and development on the critical issues which would determine the ultimate feasibility of realizing fusion power.

ENERGY RESEARCH

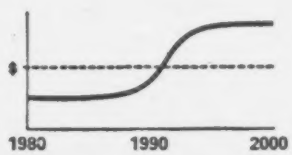
MAGNETIC FUSION ENERGY
New Strategy Accommodates Budget Constraints and Preserves Long-Term National Goal



Old

Parallel Product-Definition and Product Development Objectives

- Maintain Breadth of Reactor Concepts
- Start Significant Early Technology Demonstrations
- Build Generic Engineering Device Before Reactor Concept Selected



New

Select Product-Definition Objective

- Make Early Choices Among Reactor Concepts
- Critical Technology Issues Only
- Drop Generic Engineering Facility, Replace by Later, More Reactor-Prototypic Device

New Strategy Still Leads to Scientific and Engineering Feasibility by Turn of the Century, But With Increased Risk

SLIDE 8

Let me illustrate this by the next slide (slide 8), to make a contrast between the way in which we had planned the program up through the last 5 years and the way we are now looking at it. Over the last 5 years the fusion program has been planning on continuous growth. The goal of the program really was a combination of defining a product and simultaneously developing that product. What do I mean by product definition or product development? I indicated before that one of the basic incentives for carrying out the fusion program is its wide range of ultimate utility ranging from, central station power, to fissile fuel breeding. At this point, with our present state of knowledge, it is really not clear which is the most appropriate use for fusion. Progress in science and also the unfolding of national need will determine what that ultimate use is.

At the present time we are not sure whether the Tokamak or tandem mirror or perhaps some other concepts would be the most attractive embodiment of that useful product. So in reality, we are still in a stage of trying to understand enough about the science and technology of fusion to really understand the best ultimate use for it.

That is a product defintion.

If we understand clearly what fusion would be used for, we could go into a product development stage in which we could focus on research and development on that precise end point that we wanted to achieve.

In the philosophy that governed the program in the past we felt that the electricity generation option was sufficiently well-defined so we could use that as the focus. We set out to, in parallel, define that product a little more clearly and at the same time develop the technology and the sciences that would make it into a useful product.

In that program we were trying to maintain a breadth of reactor concepts because of the intrinsic uncertainty. We tried to start technology demonstrations early so we would have at hand that technology that allows us to go rapidly into a commercial phase. In order to integrate all energy required for the commercial application of fusion, the idea was to develop a generic engineering device which would allow us to develop fusion technology before the precise reactor product was in fact defined.

This philosophy led to a good strong program that made a lot of technical sense. However, it also required a very rapid increase in fusion funding in the near term. That funding increase is, unfortunately, incompatible with the present state of the Federal budget.

As an alternative, we have chosen the program with budgets illustrated by the lower cartoon. This is a program with constant level of effort in the near term, ramping up to a higher level of effort in the longer term.

We have separated the program into stages, the first being the definition of that product that we want to develop ultimately. We are putting off the actual focused product development until after we had more clearly defined the product.

This approach will force us to make early choices among reactor concepts. Clearly if we are going to make early choices among concepts, we must concentrate a lot of effort in developing the concepts to the point where a rational choice can be made.

It will force us to focus our attention on technology, but only on those critical technology issues that may make the difference between the product working or not working. We will have to put off demonstration at the reactor scale to the later stages of the program.

Finally, we will have to drop the generic engineering facility and replace it at a later time by an equivalent facility. Obviously, since the new facility will be started later, we will have a greater breadth of technology on which to base it. We will have a greater understanding of the scientific issues and we would hope that that engineering system would have higher performance.

We are hoping that by following this route, we can accommodate ourselves to the near term budget constraints, but that we will still focus the program on as early as demonstration of scientific feasibility and ultimately engineering feasibility as is possible.

We think, having gone over the elements of the program required to carry out this program, that we will still be able to reach the stage of engineering feasibility by the turn of the Century which is the intent of the Magnetic Fusion Engineering Act.

CURRENT TECHNICAL STATUS AND OUTLOOK OVER THIS NEXT DECADE YIELD CLEAR ORDERING AND ROLES FOR ALL CONCEPT LINES

Role	Ordering		Timing of Reactor Candidacy Readiness	Priority & Funding
Backups for Improvement and Insurance	Bumpy Torus, Stellarator, RFP		'90's	3
Developing Reactor Options	Steady State Tokamak	Tandem Mirror	Mid-to-Late '80's	2
Existing Reactor Base [+ Scientific Feasibility]	Pulsed Tokamak		Early '80's	1

SLIDE 9

The next slide (slide 9) illustrates the way in which we are ordering the research program. The fundamental basis of the success in the fusion program today is the pulsed Tokamak. That is true not only in the United States but worldwide. It is the pulsed Tokamak which provided us our basic scientific successes and the information on which we project the future progress of the program.

However, the pulsed Tokamak has some drawbacks as a practical fusion reactor. It is not impossible to turn the pulsed Tokamak into a reactor, but it would be easier if we could develop it into a steady-state device.

Fortunately, this may be possible. One of the major thrusts of our program in moving-toward a practical reactor is development of a steady-state or quasi steady-state Tokamak, something that approaches so close to steady state that we avoid engineering problems. The tandem mirror is intrinsically a steady-state device. Because it provides us with a fundamental contrast to the Tokamak and because of the scientific successes that have occurred in that program over the last few years, we feel that that also should be developed as rapidly as possible to the reactor state.

So we have in this basic element of the program, a direct competition between the steady-state or quasi-steady-state Tokamak and the tandem mirror. We hope to bring these two lines of development to reactor operation state by the late 1980's and be able to make a choice not only on the basis of theoretical projects, but on demonstrated plasma operation.

The other concepts such as the Bumpy Torus, Stellarator, and FRP, are looked on as scientific rivals of the main lines, and stimulators of new ideas. To illustrate, 4 years ago we decided to take the Bumpy Torus and try to push it toward reactor readiness. The reason for that decision was that the Bumpy Torus offered the possibility of a modular, steady state reactor. Because, I believe, from

the competition of the Bumpy Torus, the main lines have been pushed to adopt the modularity and steady state features so valuable in a practical reactor product. That is an illustration of the way in which a small research activity in these alternate lines can stimulate the main line toward greater practicality.

KEY FUSION TECHNOLOGIES CAN BE GROUPED IN THREE CLASSES

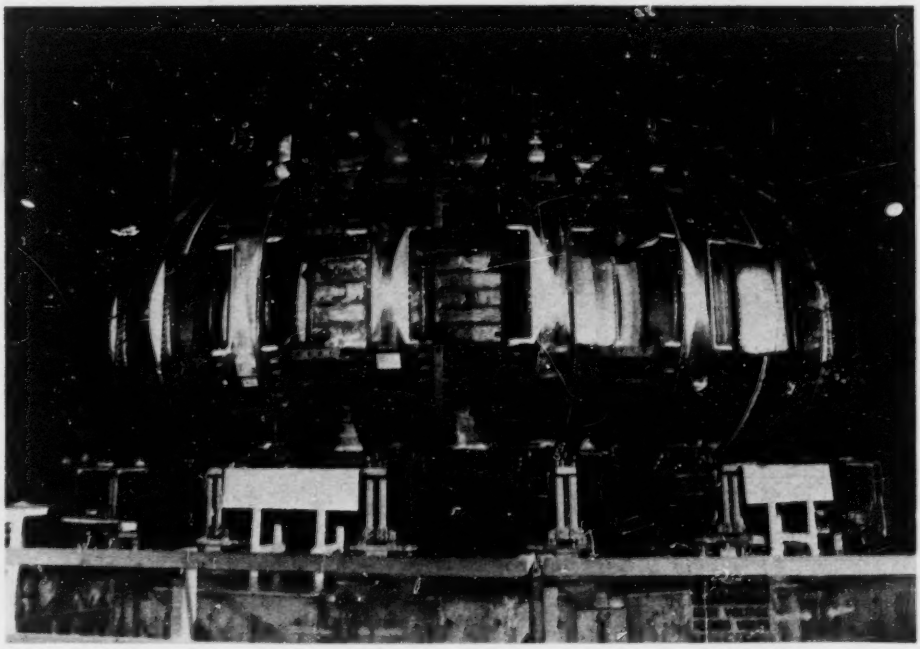
PLASMA PRODUCTION TECHNOLOGIES	NUCLEAR TECHNOLOGIES	SYSTEM TECHNOLOGIES
• MAGNETS	• FIRST WALL COMPONENTS	• INSTRUMENTATION AND CONTROL
• CRYOGENICS	• BLANKETS	• SAFETY & ENVIRONMENTAL
• R.F. SYSTEMS	• FUEL HANDLING SYSTEM	• MAINTENANCE & REMOTE HANDLING
• NEUTRAL BEAM SYSTEMS	• SHIELDING	
• PLASMA FUELING		
• POWER SUPPLIES & ENERGY STORAGE		
• PLASMA DIRECT RECOVERY		
• VACUUM		
CURRENT SCIENCE AND TECHNOLOGY FACILITIES	TFTR, TSTA FISSION REACTORS ETR	TFTR, MFTF, ETR

SLIDE 10

In terms of the technologies, (slide 10) although I indicated that we would not be able to proceed as rapidly as we had hoped, we will be able to address all of the fusion technologies in very significant ways over the next few years. We will address plasma production technologies in the large experimental devices that we have to work with in order to achieve this basic comparison between the Tokamak and magnetic mirrors. We will also address nuclear technologies. We will have programs in first wall components, fuel-handling systems and shielding. These programs will be carried out in special facilities such as the tritium test facility in Los Alamos or by using fission reactors for testing blanket elements in radioactive environments. It will also be carried out in the TFTR facility at Princeton which will be a radioactive facility. Finally, toward the end of the century, these component technologies will be integrated in an engineering test reactor.

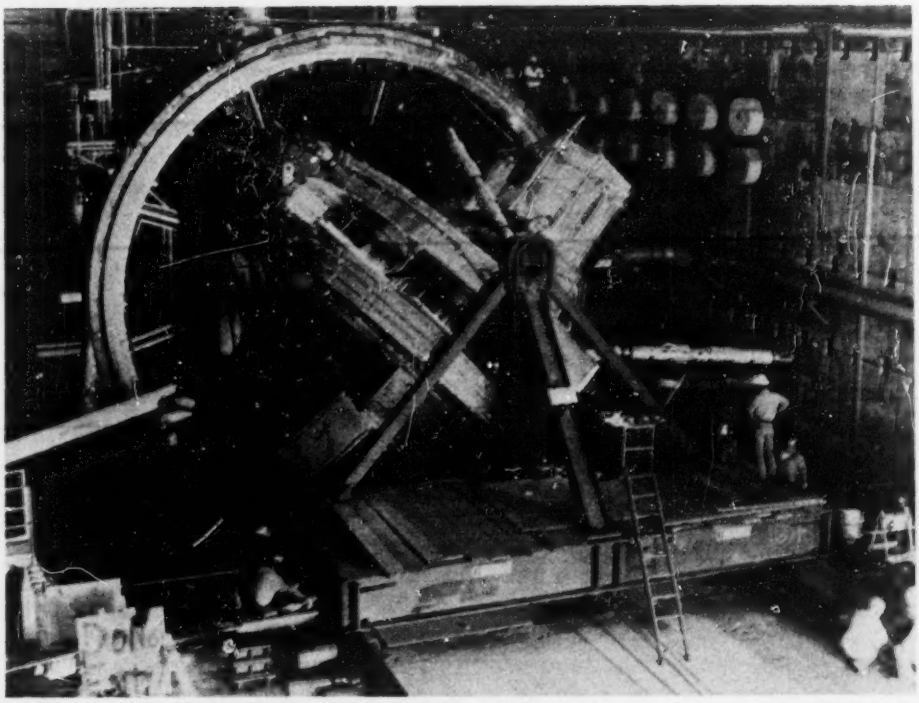
I want to emphasize we are not neglecting these technologies, we are pursuing them using the facilities at hand in the next decade. Similarly, instrumentation and control, safety and environmental

issues, and maintenance and remote handling, will be addressed in TFTR, MFTF, and finally implemented in the test reactor (ETR).



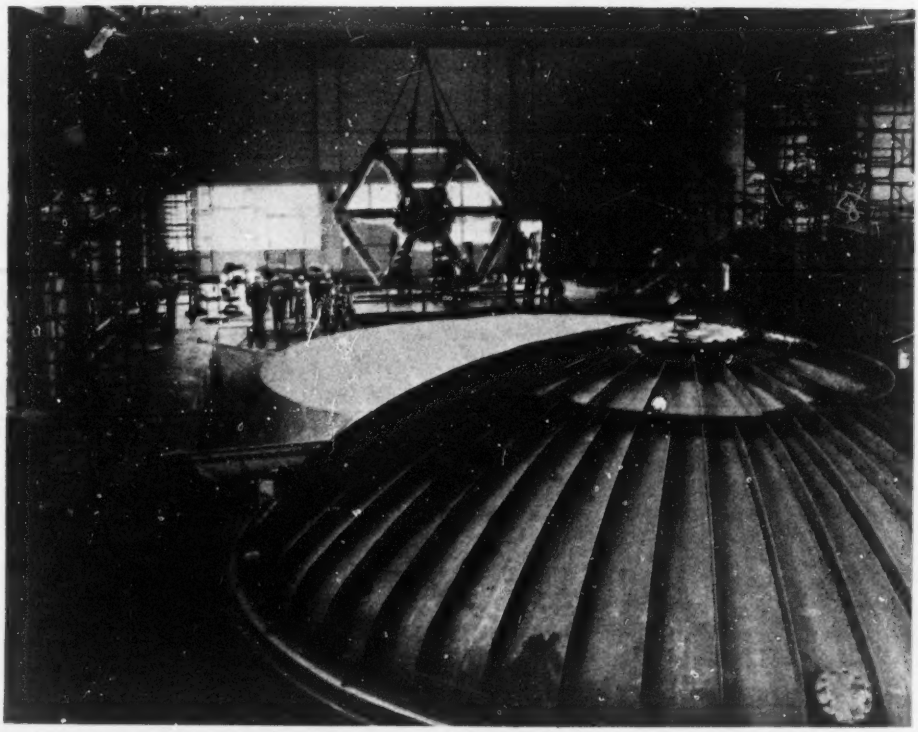
SLIDE 11

To indicate the facilities we are talking about and indicate the fusion program is by no means bereft of engineering challenges, I show you a picture of the almost completed TFTR facility at Princeton (slide 11). This is a triumph of advanced engineering. I think it is the most advanced fusion device in the world, and when completed we hope this will be the machine which will first demonstrate scientific feasibility. That demonstration will be attempted in 1986.



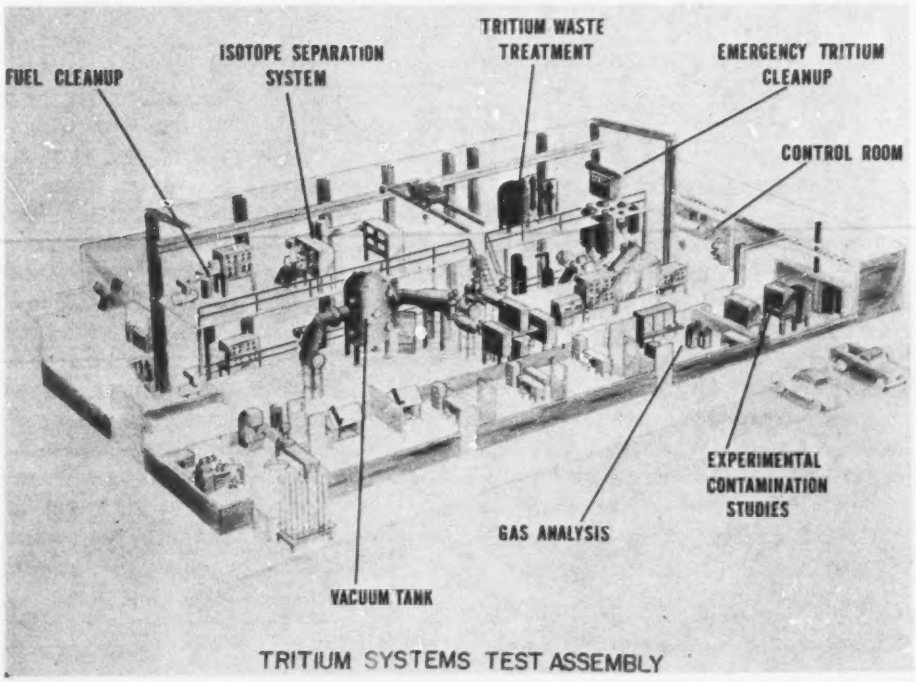
SLIDE 12

The next slide (slide 12) illustrates the MFTF superconducting magnet. This 340-ton magnet, the largest in the world, was recently successfully tested at the Lawrence Livermore National Laboratory. These are the engineering technologies that are fundamental to fusion and as you can see we are investigating them today.



SLIDE 13

The next slide (slide 13) shows a special test facility of the kind we will have to have over the next few years. This is a test facility which will test our six large superconducting coils for toroidal systems. The facility will contain three U.S. manufactured coils, and a coil from Japan, a coil from Germany and a coil from Switzerland. This is an international technology project, the product of which will be tested here in the United States beginning this year with the arrival of the first Japanese coil.



SLIDE 14

The next slide (slide 14) illustrates another, an integrated test facility. This is the Tritium Systems Test Assembly at Los Alamos which constitutes a complete, full size, tritium fuel-handling system for a fusion reactor. Again, this is very advanced technology which we are developing at this time because the safe and reliable handling of tritium is a critical feasibility issue for the practical use of fusion power. That facility will be dedicated in October.

INTERNATIONAL COOPERATION IS BEING INCREASINGLY USED
TO MAINTAIN AND EXTEND PROGRAM BREADTH

SCIENCE	TECHNOLOGY	SYSTEM INTEGRATION
• STELLARATORS (US-EC) (US-J)	• SUPERCONDUCTING COILS (IEA)	▲ INTOR (IAEA)
▲ BUMPY TORUS (US-J)	• PLASMA-WALL INTERATION (IEA)	○ ETR/FER (US-J)
▲ THEORY INSTITUTE (US-J)		▲ LARGE TOKAMAK DEVICES (IEA)
▲ REVERSED FIELD PINCH (US-EC)	▲ MATERIALS IRRADIATIONS (US-J) (IEA)	
▲ MIRROR (US-J) (US-USSR)	○ NUCLEAR TOPICS (US-J, US-FRG) (BLANKET, SHIELD, ETC.)	
▲ TOKAMAK (US-J, US-EC, US-USSR)	○ PULSED S/C COILS (US-J)	
	○ DIVERTORS (US-FRG)	

• – COOPERATION ESSENTIAL TO US PROGRAM SUCCESS IN THIS AREA

▲ – COOPERATION HELPFUL TO US PROGRAM SUCCESS

○ – COOPERATION BEING SERIOUSLY CONSIDERED

SLIDE 15

We are also making maximum use of international cooperation. This slide (slide 15) shows a summary of the array of international agreements that we have concluded over the past few years with the European Community, Japan, and the international agencies, the International Energy Agency in Paris, and the IAEA in Vienna. We also work with the U.S.S.R. through the Atomic Energy Agreement completed under President Nixon in 1974.

In some cases, these international efforts are essential to our progress. For example, in the Stellarator program we do not have a large operating experiment in this country. Both Japan and Germany have large state-of-the-art Stellarators from which we stand to gain a great deal. In other cases there is a mutual benefit but not total dependence of the United States on foreign programs. This would be the case, for example, in the mirror program where the United States has the largest and strongest mirror program. However, we get significant benefit from mirror activities in the Soviet Union and Japan. In the future, we will be trying to make the best and most practical use of all of this international cooperation.

Putting all these things together, I think that the program that we will be following in fusion will be a program that will lead to rapid progress in the field and bring us much closer to practical realization of fusion power. However, the strategy has its risks.

THE NEW STRATEGY HAS CLEAR BENEFITS AND RISKS

BENEFITS

- **MINIMIZES NEAR-TERM COST BY USING EXISTING FACILITIES AND AND MAKING EARLY CHOICES**
- **ADDRESSES TECHNOLOGY PROBLEMS CRITICAL TO ETR DECISION**
- **RETAINS SELF-SUFFICIENCY THROUGH DEVELOPMENT OF TWO MAIN LINES**

RISKS

- **EXISTING FACILITIES ARE NOT OPTIMUM FOR ALL OUTSTANDING ISSUES**
- **GREATER BURDEN IS PLACED ON ETR TO SOLVE MORE PROBLEMS BECAUSE OF GREATER SCOPE**

SLIDE 16

We can minimize near term costs by using existing facilities and making early choices. It is not pleasant, it is risky, but it is something we can do.

We can address technology problems critical to our ability to start an engineering test reactor and then develop these technologies to the reactor-compatible state during construction and in parallel with the operation of the engineering test reactor. We can retain self-sufficiency and a certain resiliency in the program by developing in parallel two lines of approach toward that reactor, the Tokamak and the mirror line.

The risks are that the existing facilities are not always the most optimum ones who could design. We think they are suitable, but not optimum. Finally, a greater burden will be placed on the test reactor when constructed. Although it will be based on a greater information base going into the project and although the design will be better, it will have to accomplish more if we are going to realize the goal of engineering feasibility by the turn of the century.

ENERGY RESEARCH

MAGNETIC FUSION ENERGY

Current Status

- Technical Progress
  - Continuous and Significant for More Than Two Decades
- Program Characteristics
  - Mature Science and Basic Technology Available
  - Highly Competent Laboratory, University, and Industrial Infrastructure
  - Major Facilities Represent Two Product Definition Lines (TFTR & MFTF-B)
- World Ranking
  - United States Leads International Program
- Risk Level
  - Scientific Feasibility Accepted; Practical Product Being Defined
- Management Approach
  - Strong Central Management
  - Highly Coordinated National Program
  - Maximum International Cooperation

SLIDE 17

So let me summarize my own view of the fusion program (slide 17). We have had continuous progress for more than two decades. I fully expect that this progress will continue. We have a mature science, that means we don't know all the answers, but we know how to frame the question and how to attack the problems. We also have the basic technology available for fusion. That technology needs a great deal of development before we can put it to practical use. We have a highly competent laboratory, university and industrial infrastructure in the program today. We would hope to preserve that infrastructure in the future.

We have major facilities representing the two main lines of attack, TFTR and MFTF-B. If pursued vigorously over the next 5 years, both of these facilities will produce reactor grade plasma which will show us in concrete ways the characteristics of Tokamak and mirror reactor plasma.

I believe the United States today leads the international program. From the International Atomic Energy Agency meeting in Baltimore over the last week, I think that is a very defensible belief. However, we have rivals on the international scene and I

think we are going to be in more of a horserace over the next decade than we have been in the past decade.

In terms of the risk level, I think most scientists today would accept that scientific feasibility will be demonstrated. However, the practical use of that demonstration is something that we still have to work on. That will be the major focus of the next decade's work.

In terms of our management approach, we will continue the strong central management that has coordinated the U.S. program and I think led to much of its success. We will seek to use international cooperation to the maximum degree possible in order to make the most rapid advance in our program.

Mr. RICHARDSON. Thank you, John.

One of the our main purposes today is to have a roundtable discussion of the program that John has outlined in the context of today's economic environment with its budgetary constraints, the environment of energy demand and energy supply and the environment of the United States participating in science on a global scale in such a way that it can continue to be perceived as the leader in that science.

Although others are scheduled to speak, the next portion of the session is to be devoted to discussion of the program that John described by participants around the table, I think we can continue this for a half an hour anyway, if interest keeps up. We might give first chance to those sitting around the table. Then if there is opportunity, we may be able to hear brief comments from the observers. Would anybody like to start out?

Mr. REICHLE. John, how do you go about making the hard choices that you mentioned? I know it is very, very difficult because many programs have sub-sub programs. They all have merit. I think you have a very, very difficult job. You must have in mind a mechanism by which you would achieve that.

Dr. CLARKE. The simple answer to that question is, "Very carefully."

We have recently put into place an advisory committee, the Magnetic Fusion Advisory Committee; that advisory structure is one of the fundamental elements in designing a program that is appropriate to make these choices.

We have asked them some very serious questions about the completeness of the mainline programs, because that is fundamental to the choice process. We have to complete those two mainline programs. Then we have asked them about the priority and the role of the alternate confinement concepts. They are presently preparing a report to comment on those things, but I think the discussions at their recent meeting have given us good ideas on how to go about making the near-term choices.

On the longer term, we have asked the National Academy of Sciences to review engineering needs of the program. I think Professor Woodson may have comments on that. But in general, I think that our approach will be to get the best advice available from independent objective groups, such as the National Academy group, from groups that are perhaps not so independent, but perhaps more knowledgable in terms of the specifics of the fusion program, such as our Magnet Fusion Advisory Committee. We will have reviews at every major decision point over the next 5 years.

I think the most important part of this process is to have the experimental hardware in place in the Tokamak and mirror program so that nature will allow us to make clear choices. Hopefully, we will not make them on a judgmental basis only, but have the data at hand to make a safe and prudent choice.

Mr. RICHARDSON. Please, anybody else? Simply speak up, without going through the formalities of recognition.

Senator?

Senator DOMENICI. Let me just ask John this question. I don't expect you to reveal the inner workings of the Department of Energy on this kind of activity versus OMB. But it does appear to me, reviewing the schedule, and the participants, if I had looked at it very carefully, I probably would have asked that we invite David Stockman here. It appears to me that OMB ends up recommending the budget in spite of what you all do in response to the question Len asked.

Could you briefly tell us how the receptivity is for this kind of long-range program? I am reminded that the administration says—in the function called energy, which has many things in it—that they want to restrain expenditures on short-term kind of research, because they assume that should be done by others rather than the Government.

Conversely, that principal emphasis should be on long-term R&D. It would seem to me that, by definition, we are talking about that kind of subject. Nonetheless, it seems that in the interest of fiscal matters, programs of this type are significantly restrained.

Could you discuss this interaction and how it goes; and what do they ask about over there in preparation for direct presentation to the President?

Dr. CLARKE. Well, I wish I could predict the action of OMB, but we are still in the early stages of discussions. The Department has just finished preparing its budget for transmittal to OMB. That was an independent process which I think has been carried out in a very businesslike fashion. So the Department will go into OMB, at least in the fusion area, with a budget that is the Department's opinion.

Through our preliminary discussions with OMB, I think the budget we are going to propose will match their expectations for this program. As you indicated, the administration's position on those programs that are Federal responsibilities applies directly to fusion. Fusion, in fact, I understand is the touchstone by which they judge other programs as to whether or not they are clearly a Federal responsibility. Fusion is clearly recognized as an important program.

The preliminary discussion we have been having with OMB is that given it is a Federal responsibility, at precisely what level should it be supported? Is that $400 million a year, $500 million, $600 million? Exactly what is it? So far that question of the level of support has been approached on a technical basis in terms of what critical facilities we actually need to accomplish the purposes I have just outlined for the program.

I can't guarantee that will stay that way when we get down to the final stages of the budget preparation in OMB. But so far, the

fusion program is being treated in a very evenhanded and professional fashion.

Dr. WATSON. John, in terms of your management approach, you stated you had a strong central management, a highly coordinated national program, and are working for more international cooperation. The thing that was missing was the role of industry.

Could you comment on how the Department and the administration see the role of industry in fusion development?

Dr. CLARKE. I am glad you asked that. I had a viewgraph on the subject which I didn't put up, because I felt that we had enough industrial representatives here that they could help me with the answer. We view the role of industry in the fusion program during this next decade as one of transition. In the past, we have had lots of involvement with industry. In terms of dollars, we spend approximately half of the total fusion budget with industry. In the past, this has really been using industry in the role of supplier of subsystems and components.

The role of industry has been very clear in the past because the fusion program was being defined and developed by the national laboratories. It is likewise very clear in the decade of the nineties, because during that period of time we will be presumably building an engineering test reactor and developing those final technologies that would be needed for establishing the commercial viability of fusion. Clearly, that is the time during which industry will be carrying the major load and major role in the program.

The problem, of course, is defining what we do during the eighties. Given a level budget for the fusion program during this period, we don't see large opportunities for industry in the program in the sense of taking over large sections of the budget. There are opportunities, and we will have to make sure that these are used, to involve industry in a substantive way in directing the course of portions of the program, and becoming sufficiently involved in the carrying out of the program in the laboratories so that they will be prepared to participate in major ways in the nineties.

The viewgraphs indicate that there are a number of issues and problems associated with this next decade. So I can't give you a precise recipe for how industry will be involved. We have discussed the possibility of involving industry in a major way in the operation of TFTR and MFTF. Industry is also involved in a major way in carrying out the engineering activities of the program through the design center at Oak Ridge. We would propose to continue these activities. I think there is a major challenge here to allow industry to participate sufficiently well in this level-funded budget period so that they will be able to take over major activities in the program during the final decade of the century.

Dr DUGAN. John, when would you make decisions on the enhanced role of industry in TFTR and MFTF?

Dr. CLARKE. One of the major issues coming up in the fusion program that will have to be resolved in calendar 1984 is the question of upgrades of the TFTR and MFTF facility. TFTR is in the final stages of assembly, MFTF is in earlier stages, but they are well established projects, with certain specific goals. Given the fact that the total investment in TFTR, including R&D and all the associated activities is in the vicinity of half a billion dollars, and that by

the time we finish MFTF we will have a similar investment, it is incumbent on us to figure out the best use for these facilities after they have accomplished their primary role. In the case of TFTR that is scientific feasibility demonstration. In the case of MFTF it is essentially feasibility of tandem mirror. The designs of those upgrades will have to be started in 1984. So that really sets the time scale for deciding in what way industry can be involved.

I think it would be very good if we could work out a mechanism whereby that upgrade process was basically the responsibility of industry, so that they could not only proceed through the design process for those upgrades, but also so that they could be involved in the initial operations of those upgrades.

Dr. SMITH. One of the things you mentioned, John, is that we have this foreign cooperation which to some degree can be an expansion of our own program. Information gained from them, and their funding is an enhancement of our program.

Can you quantify in dollars roughly, if we were to do the work we are gaining by foreign cooperation, what that would constitute if we had to do it ourselves? Is that $20 million a year, $50 million a year? What does that translate into?

Dr. CLARKE. Well, I have a concrete example. 2 years ago we concluded an agreement with the Japanese Government for support of the Doublet facility at General Atomic. That will realize approximately $70 million. That represents a $70 million investment by the Japanese Government in that facility. It has been a very successful and significant investment. It isn't just a matter of the fact the Japanese have invested in hardware for that facility. They have sent over an experimental team which has worked in parallel in a competitive way with the U.S. team. The interaction between the two teams has resulted in tremendous advancement in the program. So it is money plus the intellectual involvement.

Going back to the question of how much money is involved here, we are presently negotiating on a nmber of possibilities that I won't go into. But I think we are talking in the vicinity of $30 million to $50 million a year equivalent expenditure that we would have to put into fusion if it were not for these foreign cooperative agreements. The most serious one that we have on the table now is the question of the Fusion Materials Irradiation Test facility at Hanford in which we have had expressions of interest from both the Japanese and the European Community for participation in that project, with a remaining cost of $120 million. So these international agreements and relationships are becoming very, very important facets of our program.

Dr. SMITH. One of the things the Fusion Engineering Act requires is that this cooperation be equitable. How are you making sure that we don't get $3 and lose our shirt?

Dr. CLARKE. Again, the answer is by being very careful.

Dr. SMITH. You feel we have been able to do that?

Dr. CLARKE. Yes. We have recently prepared an analysis of a number of the international cooperative agreements that we are contemplating. This is the function of the Office of Planning and Projects that reports to me. I think we can clearly demonstrate that there is an equity involved. In fact, that is one of the funda-

mental principles of our policy on these international agreements, that there must be equity to the United States.

In the past I think the United States has been generous. We are trying to approach these fusion negotiations in a very businesslike fashion. I must say that our partners, our potential partners, appreciate that. Both Europe and Japan approach their international agreements in a very businesslike and serious fashion. So we are working with people that understand equity.

Dr. SMITH. Finally, in relationship to our program, how do companies in foreign countries fare in participation in the foreign programs?

Dr. CLARKE. Are you asking how the industry in foreign countries is involved?

Dr. SMITH. In Germany, for instance, how do German companies fare in their participation compared to U.S. companies?

Dr. CLARKE. Well, it is different in every country. I think it is well known the way that the Japanese Government involves Japanese industry. It is something that I don't think can be done in this country because of the laws and regulations that we have. But the Japanese Government is nurturing a number of Japanese industries to be the future suppliers of fusion equipment. They are some of the biggest companies in Japan, for example, Hitachi, Mitsubishi.

In Europe I think less attention is being paid to that. In the JET program, for example, all of the machine is being built in industry, but it is being built as we have used industry in the past, in terms of components. There is a greater degree of independence in terms of design in Europe. The industries do the design of the components that they supply. Then the way in which individual European countries approach it is different. In Germany it is largely through component fabrication. I think we have more participation of U.S. industry in engineering designs of future systems than Europeans do, but probably not as much as the Japanese do.

Mr. RICHARDSON. John, if I may just follow up that question. Do you know any significant difference in the motivation of your partner countries as compared to the motivation of the United States in this program? If I might guess at a motivation of the United States, it would be to create a new option for long-term energy supply.

Do other countries share this motivation exactly, or are they looking more toward commercialization, or are they looking more toward scientific discovery? What would you say about that?

Dr. CLARKE. Well, John, I wish I could capsulize the opinions of foreign nations. But there are a lot of different attitudes in these countries, just as there are a lot of different attitudes in the United States. It has often been said that the Japanese are driving toward commercialization. That may be the case. I have seen some statements from the Japanese Government that that is the ultimate goal.

But obviously that is also the ultimate goal of the U.S. program. In terms of the way that they operate the program there is the difference, I just noted, that they have selected certain industries to become fusion experts. Whether you take that as evidence that

they are trying to commercialize fusion or that that is just the way they operate, I really can't say.

There is a difference, I think, between the U.S. program and the Japanese program and the European program. Both the United States and Japan have a more practical orientation to their fusion programs than the Europeans. This is perhaps due to the fact that most of the European program is carried out in plasma physics laboratories, and there is a very heavy cast of plasma physics concentration in the European program. We have, in the United States, paid much more attention to technology and engineering development than the Europeans, but perhaps not as much as the Japanese. I think there is a gradation from East to West in this particular case.

Mr. MILLER. John, given the growing cooperation between the United States and other foreign nations, do you sense any concern about the challenge and possible competition that these other countries may bring to bear?

Dr. CLARKE. Do you mean with respect to our fusion program?

Mr. MILLER. Yes.

Dr. CLARKE. Well, let me just say frankly that I think things are changing on the world scene. We have been challenged in the past by the Japanese in the sense that they started with a very small program, and they have been working very, very hard over the past 5 years to bring their program up to an internationally competitive level. They have been increasing their budget at an extremely fast rate every year, massive increases.

However, there is evidence from Japan at the present time that this period of growth is over. In terms of Europe, the program has been adequately funded, but without spectacular growth over the past few years. Recently they recognized that they are very weak in the area of fusion technology, and they are beginning to put more money into it, but not in a massive way.

I think the realization has settled over the world that fusion is a program for the long term and that massive increases of money beyond a certain critical level really won't bring that date very much closer. So if you are asking whether or not we feel pushed by the Japanese and the Europeans, no I wouldn't say we do at the present time. I think the mood right now internationally seems to be one of a sober, businesslike attitude. There is a job to be done here. We need a certain level of resources to do the job and we are just about there internationally.

In terms of Japan, Europe and the United States, we spent approximately $1.1 billion or $1.2 billion a year on fusion. That is a very large investment worldwide. I think there is a feeling in these countries that that is approximately the right level for the programs.

Mr. MILLER. Do we know how that compares to level of effort in the Soviet Union?

Dr. CLARKE. It is very difficult to say because their budget figures aren't available. They probably have more people working than the United States program, but I think they spend less money because they aren't as facility oriented as we are. But it is close.

Dr. WATSON. I was wondering if I could get the industrial view. Would you agree with the EPRI's position on industry's role, or do

you feel industry now is ready to take a larger role? And if so, what would that role be?

Mr. GAVIN. I am not sure we are prepared to say precisely what role industry should strive for at this point. I think the time schedule is such that much of this will work out very naturally. It has been obvious over the last several years that in the industry, in our case in particular, the aerospace industry, there are basic skills which fit well into this overall undertaking. While it hasn't been highlighted here, I think the work that has been done, the systems engineering efforts to define, I am not sure what is the next step, has made a difference.

The fact is that some of the industrial talent is being applied to the systems engineering aspect of the future devices. I think that is a proper thing to be doing in parallel with some of the experiments that are actually taking place at some of the facilities. I think the systems engineering aspect of the ultimate product is of extreme interest. Certainly its success, whether it is a matter of optimization, reliability, cost per unit of output, will be a product of successful systems engineering.

We are not really at the point of proceeding seriously with what I think of as the final systems engineering. As John pointed out, there are things that are going to be coming forward from current experiments that modify some of the aims we have. So right now I think we have a reasonable balance of the interaction of the skills of industry with the agency. What I do feel is lacking—and I am going to pursue a tack I have taken publicly before—it seems to me that the thing that is missing here in the United States is a recognized national priority for a long-term project, so that in a sense the things which take 5 and 10 years to do receive a little better protection from the annual budget debate.

I think we need, quite frankly, a Presidential commitment. By saying that I don't mean more money this year or next year or the year after. What I do suggest is that there seems to be a need for a nationally established priority that says that we think in the long run that fusion power is going to be very important, and that we are willing to devote a lot of our energy to it.

There are two programs in the past that I think might provide lessons for laying out the fusion program. One which hasn't been highlighted very much can probably be spoken to better by somebody from Boeing. The reason the world travels on American airlines today is because of the interaction between the old NACA and the U.S. Air Force over a long number of years before it culminated in the technology which we take for granted today.

Indeed, there was a transition in that case between Federal funding and private funding. I am not saying that that transition is precisely the pattern which should be followed here, but it might be illustrative to go back and look to see how that happened.

The second example which might be useful to look at is the Apollo program. The Apollo program never reached the point where private industry made sizable investments in commercial ventures. It did, however, provide, I think, some lessons in how a program that had a national priority operated and directed from the agency charged with its responsibility and how that agency in-

teracted with industry in order to assemble the necessary teams to carry out the program.

I think between those two quite different examples, there are some lessons to be learned that could be applied to this program. I don't think any of us sitting here today would say that in 1984 a certain task should become a problem of industry as opposed to a Government agency. But I suspect that for a good many years to come there is going to be an involvement of not only the Government agency and industry, but I think of the Government agency as being two parts. There is the central leadership here in Washington, and of course there are the national laboratories which must continue to have a major role in this thing for it to be done successfully.

Dr. WATSON. Leonard?

Mr. REICHLE. I would certainly agree with Joe in terms of our trying if at all possible to get a Presidential commitment. Not only from the point of view of funding, but in terms of the establishment and achievement of national goals. If we can do that it seems we could find ways and means of getting this problem to the President in the same way that the problem was recognized by the Congress 2 years ago in passing the Fusion act of 1980.

On the question of industry relationships, I think the fusion program has come a long way. It started with a small and then growing scientific base, most of which is in the national laboratories and universities. Gradually as we have gotten to the developmental stage and the component systems stage as Joe mentioned, we have gotten industry involved. I think two principles might be important to consider. One is that we recognize each way where our greatest strengths are.

On the scientific side, it is laboratories and universities, and industry, for the systems engineering, component development and manufacture. Second, we should try to work out the kind of relationship that the Japanese seem to have between their industry, universities, their laboratories, their government. So to the extent we can develop again this theme of a closer interrelationship I think our program would be well off. In gradually pushing more things to industry, I think another consideration might be that we should try to push both management and technical direction, even if it is small pieces, to industry.

Now there has to be the Government, the laboratory, the university, overall cognizance with them. But let's make industry perform in the assignments given to them and then encourage them to perform more of these kinds of things in a close working relationship with DOE, laboratories and universities.

Mr. RICHARDSON. Steve?

Dr. DEAN. I will make a few comments. I think that the program that has been laid out by John is a good one based on the assumptions under which he is constrained to operate, which he stated at the beginning as being flat budgets through 1988, and then a very rapid buildup after that. The net result of that it seems to me is to put the role of industry that was envisaged in the act off 10 years. The role John described as taking place in the 1990's was envisaged by the act to occur in the 1980's. I think that is unfortunate.

Not being a member of the executive branch, I don't personally have to take as God-given the assumption of flat budgets through 1988 and the promise of a rapid ramp-up thereafter. I don't think that is realistic. In fact, if flat budgets go through 1988, I don't have any reason to believe that in 1988 all of a sudden everybody changes their minds and ramps up then.

So I believe that we have to question, and I hope the Congress would question, this assumption of flat budgets. The act approached the problem from a different point of view. The act said "Let's try to achieve a certain level of objective by a certain date. Then let's look and see what it costs. If it costs too much, we could go back and change the objective slightly and recompute the cost." It is not necessary to go all the way to the other extreme, it seems to me, and assume that the budgets are going to be flat for a long period of time, and then lay out an entire program based on the consequences of that assumption. So from that point of view I have to take a different stance than I think is logical for the Department to take, since they have to act under 5-year planning guidance that they have received from the OMB.

In response to a question that you asked earlier, Senator, about the attitude at the OMB, which I think is dominated at the present time by Dr. Keyworth's office, I think there has been a fundamental change in the view there as compared to what is in the act.

At a recent meeting we had John Marcum, who works for Dr. Keyworth, state that in their view there was an adequate supply of fossil fuels, et cetera, for a long period of time. And that therefore there was no urgency to the development of fusion and that we really didn't need fusion for a long time. I believe that that is the fundamental policy assumption that is being made that leads them to say let's have flat budgets, because there is no rush to get on with this engineering job.

Now that was not the assumptions made in laying out the Fusion Act. In fact, in the hearings that led up to that act, the Department of Energy actually was asked a question as to whether in the long term it would cost more or less to develop fusion more aggressively in the near term as opposed to developing it more leisurely in the near term and going faster later. And they acknowledged that in fact it would be cheaper overall in the long term to proceed more aggressively in the short term, and I subscribe to that point of view.

So, whereas I congratulate John for doing the best that possibly could be done in under a flat budget assumption, I hope that the country will not simply accept that assumption. And there is some evidence that even internally this summer in the Department's request to OMB they haven't accepted their own assumption; they haven't fully accepted that strategy themselves but, in fact, are expecting to ask for some growth. So I hope there is some intermediate case here that would allow industry to play a more responsible role earlier than is laid out in tnis chart.

Mr. RICHARDSON. I have to digress a minute because I have made some assumptions about our time budget. I have assumed that Herb Woodson may need a half hour to make his points. Similarly, Ron Davison a half hour; and, similarly, Bob Scott and Frank Graham taken together, perhaps three quarters of an hour. If those

assumptions are correct, we can afford a 10- or 15-minute break at this point.

Would you tell me if my assumptions are on the mark?

Dr. DUGAN. That is allowing for discussion?

Mr. RICHARDSON. Yes. Lunch is a boundary condition at 12:30. I think people would appreciate a break. I also think that John's presentation of the program which is the centerpiece of this meeting and the discussion of it has been most valuable. I don't want to cut that off, nor do I want to shortchange our speakers.

With your agreement, then, I would propose that we help ourselves to coffee and the remaining donuts for about 10 minutes and promptly reconvene.

[Recess.]

Mr. RICHARDSON. OK. We are all settled again.

Mr. GAVIN. May I make a comment before we proceed?

Mr. RICHARDSON. You surely may.

Mr. GAVIN. I would like to underline one of the comments Steve made just before we broke—Steve Dean. He commented on the fact that there are people who have great confidence in the vast supply of coal that sits in this country. He didn't quite say what I would like to say. That is, frankly, I just don't think it is going to be acceptable to burn coal for as long or in as large quantities as some of the projections would lead you to think might be feasible.

We are at the point where either from acid rain or from CO_2 content in the atmosphere, there is going to be a demand for cutback of burning coal. So I think it is false assurance to look at that 400-year supply of coal.

Mr. RICHARDSON. Joe, I think you are right. I think in addition to the points you mention, one has to look at the safety aspects of the coal cycle, all the way from extraction to consumption.

Let's move on to our next speaker, who is Herb Woodson. I have had the privilege of working with Herb over the last 6 months or so in the National Academy of Science's effort. I would say Herb appears to be MIT through and through, having been educated with about three degrees from there, and having been so popular that he stayed on the faculty for about 15 years.

But, then, Texas apparently was successful in bidding for him, and he moved to Austin to the University of Texas in 1971. He served 10 years as chairman of the electrical engineering department. His present position is a dual position. First of all, he is director of the Center for Energy Studies at the University of Texas, and he tells me just this morning that he is the new occupant of a chair in engineering, in particular, the chair endowed in the name of Ernest H. Cockerell at the University of Texas.

Herb is going to give some informal views deriving from the discussions and presentations that we have had at the National Academy of Sciences.

STATEMENT OF DR. HERBERT H. WOODSON, CHAIRMAN, COMMITTEE ON MAGNETIC FUSION, ENERGY ENGINEERING BOARD, NATIONAL RESEARCH COUNCIL

Dr. WOODSON. Thank you, John. Just so that I do this correctly, I have prepared a presentation which I think Nancy Jeffery has had

reproduced and distributed to most people. I will save my misstatements for the discussion later, and read this to you just to make sure that I say it the way I intended to at the outset.

I discovered that there is already some disagreement in the audience with some of the things that were said. So I am sure that will come out in the discussion later. But let me go through this, and then we can get to elaboration to the extent that it is worthwhile.

I am making this presentation as chairman of the Committee on Magnetic Fusion of the National Research Council. Our committee, a roster of which is given in attachment A, and let me tell you who they are. They include Dr. Everett Bloom, Dr. Gottlieb, Dr. John Landis of Stone & Webster, Bill Manley, Bill Stacy of Georgia Tech, and John Steckley of Magnetic Corp. of America. That is the committee.

This committe was established primarily to carry out the task described in attachment B to this. I will talk about the task in a minute. The workshop called for in the "Statement of Task" was held on August 3 and 4, 1982, here in Washington, and the report is in the very early stages of preparation. As a consequence, our report has not proceeded far enough to develop findings that I can report on here. Rather I will give here a progress report and attempt to summarize the views presented during the workship and the reactions to and discussions of those views by the attendees.

Any conclusions that appear to be reached will be mine alone and not those of the commttee or the National Research Council. Now to the task.

As described in attachment B, the Committee on Magnetic Fusion was established to plan and conduct a workshop, I might add with funding from the Department of Energy, with three objectives:

One, determine how clearly the future engineering development needs of fusion can be specified;

Two, determine how, with an engineering development plan, the engineering development capacity of industry can be effectively coupled to the ongoing scientific development in laboratories and universities; and

Three, prepare a report delineating the findings and discussions of the committee.

To best meet the objectives, the committee organized a 2-day workshop composed of the presentations identified in attachment C. That is a program for the workshop, and you can look at that for detail, if you are curious. These presentations include technical detail on the different fusion reactor concepts and their engineering development needs in terms of subsystems, the needs for system integration, and the modes of participation of different components of industry in the fusion program.

Let me digress for just a moment and read from attachment B, and indicate that it says the committee will solicit assistance from relevant parts of the National Research Council and other organizations in the public and private sectors. The workshop will provide a forum for dialogue between knowledgeable fusion technologists and senior experienced technologists from fields involving related technology such as aerospace technology, electronics, robotics, and nuclear and electric power systems.

So, to assist the committee in its work, a number of senior engineers who have substantial and significant contributions in fields other than fusion were invited to attend the workshop, to participate in the discussions, and then to provide their reactions to the committee both verbally at a committee meeting on the day after the workshop concluded, and in writing later. These engineers, identified in the roster in attachment D, participated actively and were most helpful in providing thoughful comments that will be of great value to the committee as it develops its findings.

Now, to the engineering development needs. The presentations and discussions of the workshop indicated that a large number of engineering development needs of fusion have been identified through conceptual reactor design studies. The most effort has been expanded on Tokamak-based systems, and those conceptual designs are quite detailed and mature. Less has been done on mirror, EBT, and reversed-field pinch reactors, but conceptual design work on these systems is continuing.

Because of the preponderance of work on reactor designs for Tokamak-based systems, most engineering needs are defined in terms of Tokamaks; however, many engineering development needs have been identified as generic to most magnetic-confinement fusion concepts and hence are needed no matter which confinement concept is finally chosen.

The engineering development needs were identified and discussed in the workshop in terms of two major sets of subsystems: the plasma subsystems and the nuclear subsystems.

The plasma subsystems were identified as those needed to heat, confine and maintain the purity of a fusion-grade plasma. The major technologies and their development needs for plasma heating were identified as—and I would indicate these are not listed in order of importance, but in order of having been addressed in the workshop. First, the ohmic heating of a Tokamak needs superconductors that can perform satisfactorily under the transient conditions required.

Second, neutral beam injection needs less expensive and more efficient beam generation including negative-ion-produced beams at the higher injection energies.

Finally, radio frequency heating and current drive need sources developed for the high frequencies associated with electron cyclotron resonance heating [ECRH] and transmission and launching to couple the rf power efficiently to the plasma while isolating the rf source from the neutrons emanating from the fusing plasma at all frequencies from the relatively low frequency required for ion cyclotron resonance heating [ICRH] through the intermediate frequency needed for lower hybrid resonance heating [LHRH] to the relatively high frequency for ECRH.

The technologies involved in plasma confinement are those necessary for the production of high magnetic fields in large volumes with little expenditure of power. This requirement is generally thought of as being fulfilled by superconducting coils, which translates to better superconducting materials, methods of fabrication to withstand forces both steady and cyclic, insulators and other materials that can tolerate bombardment by neutrons, and the refrigeration systems necessary to maintain the cryogenic environment.

Maintaining plasma purity requires removing part of the plasma to separate the ash and recycle unburned fuel to the reactor, and insuring that impurities are not allowed to enter the plasma. All of these requirements translate into technology requirements for materials that can handle unusually high particle and heat fluxes.

The successful development of technologies to satisfy these requirements suffers severely from the lack of facilities that produce reactor-grade plasmas that can be used to develop experimentally the data bases needed to make choices and evaluate options.

The nuclear subsystems were defined and discussed in the workshop under four categories:

Blanket technology involves materials and systems for breeding tritium for fuel, recovering fusion energy as heat and structural materials.

Tritium systems are those required for recovery and control to provide fuel for the reactor.

Materials are needed with the right physical, chemical, and mechanical properties to perform all the needed functions in a fusion environment, which is markedly different from that in any other energy system.

The materials problems resulting from plasma-wall interactions are so unique and severe that they are identified separately from other materials problems.

Although the engineering development needs in the nuclear subsystems are well defined, the facilities for accomplishing those developments are severely limited. There is a strong need for a fusion environment—including neutrons, particles, and radiation—of sufficient strength and duration to develop the experimental data bases needed.

As part of the engineering development needs, the workshop examined the need for system integration. Presentations indicated the high importance of system integration in terms of compatibility of subsystems, ease of construction and testing, and need for remote maintenance.

Now to the participation of industry, if I may.

The presentations at the workshop indicated that although involvement is essential to the successful commercial adoption of fusion, industry will not invest large sums of its own money until the probability of a return on that investment is higher and nearer term than it appears to be for fusion at the moment. Nevertheless, the long-term importance of fusion is so great that industry will allocate people and resources to fusion projects under Government contract with moderate cost sharing in some cases.

Although not included in the statement of task for the workshop, several other issues were raised during the workshop that are worth noting here.

First, it was evident from the workshop presentations that over the past two decades the fusion community has developed a wholly new scientific enterprise and made great strides in its development and understanding. These accomplishments are testimony to the dedication and high competence of the participants.

In spite of the significant accomplishments of the recent past, considerably more remains to be done in both scientific understanding and technology development before commercial applica-

tions of fusion can be identified and engineered. Thus practical applications of fusion appear to be more than two decades in the future, and a sustainable and productive research and development program needs to be defined to reach practicality.

The view was expressed during the workshop that it may be too early in the development of fusion to select a pure-fusion, DT-fueled, electric generating plant as the primary objective of the program. The basis for this view was that a pure-fusion electric generating plant may be the most difficult application of fusion to develop and hence may take longer than other applications, such as the production of neutrons for the breeding of fissionable fuel. Another basis for this view was that fusion is a new and different science still in development and may yield yet-unthought-of opportunities

Just to summarize; the workshop conducted by the Committee on Magnetic Fusion of the National Research Council included presentations by the fusion community on the state of development of fusion and the description of its engineering needs to an audience of engineers and scientists from many fields. There were presentations concerning the proper role of industry in the fusion program. The reaction seemed to be that fusion has made great scientific and technological strides in the recent past, but more are needed and will come. At this stage the engineering needs are well defined, but those needs may change as the science advances.

Industrial participation in fusion is currently in the form primarily of contracted support. Substantial capital investments by industry will likely not come until fusion science and technology have advanced significantly further.

Although fusion science and technology have enjoyed tremendous advances recently, the commerical applications of fusion appear to be several decades in the future and uncertain in concept. Nonetheless, fusion has such great promise for the long-term future that a productive program needs to be continued.

These are my impressions of what was covered in our workshop. I remind you that our committee is in the early stages of preparing a report on the material developed during the workshop, and after we complete our report it must go through the National Research Council review process before it becomes official and is made public. That should occur sometime after the beginning of 1983.

Thank you and I will be glad to answer any questions you may have.

[The prepared statement of Dr. Herbert H. Woodson follows:]

PRESENTATION BY HERBERT H. WOODSON

INTRODUCTION

I am making this presentation as Chairman of the Committee on magnetic Fusion of the National Research Council. Our Committee, a roster of which is given in Attachment A, was established primarily to carry out the task described in Attachment B. The workshop called for in the Statement of Task was held on August 3, and 4, 1982, and the report is in the very early stages of preparation. As a consequence, our work has not proceeded far enough to develop findings that I can report on here. Rather, I will give here a progress report and attempt to summarize the views presented during the workship and the reactions to and discussions of those views by the attendees. Any conclusions that appear to be reached will be mine and not those of the Committee or the National Research Council.

THE TASK

As described in Attachment B, the Committee on Magnetic Fusion was established to plan and conduct a workshop with three objectives:

"1. determine how clearly the future engineering development needs of fusion can be specified;

2. determine how, with an engineering development plan, the engineering development capacity of industry can be effectively coupled to the ongoing scientific development in laboratories and universities; and

3. prepare a report delineating the findings and discussions of the committee."

THE WORKSHOP

To best meet the objectives, the Committee organized a two-day workshop composed of the presentations identified in Attachment C. These presentations include technical detail on the different fusion reactor concepts and their engineering development needs in terms of subsystems, the needs for system integration, and the modes of participation of different components of industry in the fusion program.

To assist the Committee in its work, a number of senior engineers who have made substantial and significant contributions in fields other than fusion were invited to attend the workshop, to participate in the discussions, and then to provide their reactions to the Committee both verbally at a Committee meeting on the day after the workshop concluded, and in writing later. These engineers, identified in the roster in Attachment D, participated actively and were most helpful in providing thoughtful comments that will be of great value to the Committee as it develops it findings.

ENGINEERING DEVELOPMENT NEEDS

The presentations and discussions of the workshop indicated that a large number of engineering development needs of fusion have been identified through conceptual reactor design studies. The most effort has been expanded on Tokamak-based systems and, those conceptual designs are quite detailed and mature. Less has been done on mirror, EBT, and reversed-field-pinch-reactors, but conceptual design work on these systems is continuing.

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Ohmic heating needs superconductors that can perform satisfactorily under the transient conditions required.

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Although the engineering development needs in the nuclear subsystems are well defined, the facilities for accomplishing those developments are severely limited. There is a strong need for a fusion environment (including neutrons, particles, and radiation) of sufficient strength and duration to develop the experimental data bases needed.

As part of the engineering development needs, the workshop examined the need for system integration. Presentations indicated the high importance of system integration in terms of compatibility of subsystems, ease of construction and testing, and need for remote maintenance.

PARTICIPATION OF INDUSTRY

The presentations at the workshop indicated that although industry involvement is essential to the successful commercial adoption of fusion, industry will not invest large sums of its own money until the probability of a return on that investment is higher and nearer term than it appears to be for fusion at the moment. Nevertheless, the long-term importance of fusion is so great that industry will allocate people and resources to fusion projects under government contract with moderate cost sharing in some cases.

OTHER CONSIDERATIONS

Although not included in the statement of task for the workshop, several other issues were raised during the workshop that are worth noting here.

First, it was evident from the workshop presentations that over the past two decades the fusion community has developed a wholly new scientific enterprise and made great strides in its development and understanding. These accomplishments are testimony to the dedication and high competence of the participants.

In spite of the significant accomplishments of the recent past, considerably more remains to be done in both scientific understanding and technology development before commercial applications of fusion can be identified and engineered. Thus practical applications of fusion appear to be more than two decades in the future, and a sustainable and productive research and development program needs to be defined to reach practicality.

The view was expressed during the workshop that it may be too early in the development of fusion to select a pure-fusion. DT-fueled, electric generating plant as the primary objective of the program. The basis for this view was that a pure fusion electric generating plant may be the most difficult application of fusion to develop and hence may take longer than other applications, such as the production of neutrons for the breeding of fissionable full. Another basis for this view was that fusion is a new and different science still in development and may yield yet-unthought-of opportunities.

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NATIONAL RESEARCH COUNCIL
ASSEMBLY OF ENGINEERING
2101 Constitution Avenue Washington, D. C. 20418

ENERGY ENGINEERING BOARD (202) 334-3344

Attachment A

COMMITTEE ON MAGNETIC FUSION

April 1982

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Attachment 3 Code Designator for Group Described: CMF

Assembly of Engineering
ASSEMBLY or COMMISSION

Committee on Magnetic Fusion
COMMITTEE

Energy Engineering Board
DIVISION, OFFICE, or BOARD

SUB-UNIT

Staff Officer: Dennis F. Miller

STATEMENT OF TASK

(Make clear what is expected of the group described. Include any contractual obligation under which the group bears responsibility. Limit to not more than this page.)

The committee will plan and conduct a workshop which will have three objectives:

1. determine how clearly the future engineering development needs for fusion can be specified;

2. determine how, with an engineering development plan, the engineering development capacity of industry can be effectively coupled to the coincident ongoing scientific development in national laboratories and universities: and

3. prepare a report delineating the findings and discussions of the committee.

The committee will solicit assistance from relevant parts of the National Research Council and other organizations in the public and private sectors. The workshop will provide a forum for a dialogue between knowledgeable fusion technologists and senior experienced technologists from fields involving related technology, such as aerospace technology, electronics, robotics, nuclear and electrical power systems.

The committee will consist of individuals familiar with the fusion energy issues. The committee will set the guidelines for the conduct of the workshop, develop a roster of participants, and prepare the summary of the workshop discussion. The participants will consist of engineers and scientists from laboratories, industries, universities, government, and other pertinent organizations.

The study is supported by a contract from the U.S. Department of Energy.

March 1, 1982
(date of statement)

NATIONAL RESEARCH COUNCIL
COMMISSION ON ENGINEERING AND TECHNICAL SYSTEMS
2101 Constitution Avenue Washington, D.C. 20418

ENERGY ENGINEERING BOARD

Attachment C

July 30, 1982

WORKSHOP ON THE FUTURE ENGINEERING DEVELOPMENT NEEDS

OF MAGNETIC FUSION

Room 451, Joseph Henry Building*
2100 Pennsylvania Avenue, N.W.
Washington, D.C. 20418

August 3-4, 1982

TENTATIVE PROGRAM

Tuesday, August 3, 1982

8:30 a.m. Coffee and Pastries

9:00 Introduction. OBJECTIVES OF THE WORKSHOP H. Woodson

9:10 Opening. CURRENT STRATEGY IN THE FUSION PROGRAM
John F. Clarke, Office of Fusion Energy

9:30 Session 1. SYSTEMS DEFINITION
W. M. Stacey, Jr., Organizer

A. Near-term Tokamaks
Donald Steiner, Renesselaer Polyechnic Institute

B. Commercial Tokamaks
R. W. Conn, University of California at Los Angeles

Break

C. Magnetic Mirror Machines
K. I. Thomassen, Lawrence Livermore Laboratory

D. Other Concepts
R. A. Krakowski, Los Alamos National Laboratory

12:45 p.m. LUNCH - Buffet Style

Telephone contact for the meeting: John M. Richardson (202) 334-3344

The National Research Council is the principal operating agency of the National Academy of Sciences and the National Academy of Engineering to serve government and other organizations

Tuesday, August 3, 1982 - Continued

1:45 Session 2. SUBSYSTEM TECHNOLOGY, I: PLASMA SUBSYSTEMS
Melvin B. Gottlieb and Z. John Stekly, Organizers

A. Magnetics
D. Bruce Montgomery, Massachusetts Institute of Technology

B. Heating
John Rawls, General Atomic Corporation

C. Associated Impurities and Control
John A. Schmidt, Princeton Plasma Physics Laboratory

Break

4:00 Session 3. SUBSYSTEM TECHNOLOGY, II: NUCLEAR SUBSYSTEMS
Everett Bloom and William D. Manly, Organizers

A. Blanket Technology
Dale L. Smith, Argonne National Laboratory

B. Tritium Recovery
James Anderson, Los Alamos National Laboratory

5:30 Recess

6:00 RECEPTION AND COCKTAILS
Rotunda, National Academy of Sciences Building
2101 Constitution Avenue

Wednesday, August 4, 1982

8:20 a.m. Coffee and Pastries

8:30 Session 3. SUBSYSTEM TECHNOLOGY, II: NUCLEAR SUBSYSTEMS (continued)

C. Materials
R. E. Gold, Westinghouse Electric Corporation

Wednesday, August 4, 1982 - Continued

D. Plasma-Wall Interaction
Walter Bauer, Sandia Laboratories (Livermore)

Break

10:15 Session 4. SYSTEM INTEGRATION
John W. Landis, Organizer

A. The Importance of Systems Integration and Maintenance to the Development of the Tokamak Configuration
Charles A. Flanagan, Fusion Engineering Design Center

B. Remote Maintenance and System Integration of Fusion Devices from the Users' Standpoint
Dwain F. Spencer, Electric Power Research Institute

C. Primary Problems in the Integration, Testing and Construction of Fusion Power Plants
William J. L. Kennedy, Stone and Webster Engineering Corporation

12:30 p.m. LUNCH - Buffet Style

2:00 Session 5. PARTICIPATION OF INDUSTRY
John W. Landis, Organizer

A. Planning the Program to Fit the Goals
Joseph E. Gavin, Grumman Corporation

B. Utility Participation in Magnetic Fusion Programs
Betty K. Jensen, Public Service Electric and Gas Company (Newark)

Break

C. Magnetic Fusion--Industrial Participation, the Bridge to Ultimate Success
John B. Yasinsky
Westinghouse Electric Corporation

5:00 ADJOURNMENT

Attachment D

SENIOR ENGINEERS INVITED TO PARTICIPATE IN

WORKSHOP AND PROVIDE REACTIONS TO COMMITTEE

Paul F. Chenea
Retired Vice-President
General Motors Corporation

William A. Frederick
Pennsylvania Power &
Light Company

John R. Gilleland, Director
Doublet III Division
General Atomic Company

Martin Goland, President
Southwest Research Institute

Walter F. Leverton
Aerospace Corporation

Clifford D. Marks
Vice-President
McDonnell Douglas Corporation

James W. Moyer, Manager
Physical Systems R & D
Southern California
Edison Company

Bernard M. Oliver
Vice-President for R & D (retired)
Hewlett-Packard Company

Heinz G. Pfeiffer
Pennsylvania Power &
Light Company

Charles E. Reed
Retired Senior Vice-President
General Electric Company

D. E. Simpson
Vice-President
Westinghouse Hanford Company

John W. Simpson
Retired President
Westinghouse Power Systems Company

Dr. ROSENTHAL. In development of new technologies, there generally appears a time where we get optimistic and think we are going to succeed. Then we recognize we need all sorts of things and we undertake what we describe as generic technology development. Later on we face budget realities and decide what we want to do is develop the things we need at the time we need them. John Clarke has indicated the change that we may be taking in the direction of the fusion program is to concentrate on the things we have to have. Is your committee going to address that question of what ought to be done in generic ways and when we need it and so on, whether or not we have the money?

Dr. WOODSON. One has to realize that this workshop did not involve a lot of manhours and time, but rather presentations from the community and sort of off the top of the head reactions from the people in attendance.

I think that what our report will conclude is kind of an overview assessment of how well the needs are defined and possibly the suggestions of some areas that ought to be looked into more deeply by a committee of similar breadth of background.

So, I don't think our workshop is in any position to essentially define in detail any kind of a program that ought to be carried out with respect to budgets or anything else. But rather to give its impression of how realistic and completely things are defined at the moment and directions that one might take to look more deeply and to help plan the program.

Ms. FREIWALD. Along that same vein, I was struck by your comments about the need for facilities. Did you think about—did your group think about what direction we ought to go in order to provide those facilities and how existing facilities can be supplying that need? Did you think about the role of some of the facilities we have like the TSTA or FMIT?

Dr. WOODSON. Those issues were certainly discussed and I would say that if you counted the number of people in attendance at the workshop, that is about the number of opinions that are expressed.

So it is difficult to develop what the conclusion of the workshop was. I think this was essentially an engineering exercise. I think that, as engineers, we feel strongly that before you get to the point of really commiting yourself to design and build a major facility, you want some good hard test data on which to base the design.

We are troubled by the fact that in the very important materials area, for example, there simply is not a fusion source with which to test the materials first hand at this time. There will be some day, but until we have that, we won't have the kind of tests needed. With respect to FMIT, I think it could play a useful role, but I think it was also—this is my opinion—it was the conclusion of our workshop that it is only a partial measure in solving the problem that we need to solve.

Ms. FREIWALD. Thank you.

Dr. WOODSON. The problem is that in order to develop applications of fusion, we need some real fusion to work with as engineers.

Mr. GRAHAM. I have a question for you, Herb. The Buchsbaum panel looked at institutional arrangements for developing an engineering technology and recommended the Center for Fusion Engi-

neering which again was incorporated in the Magnetic Fusion Engineering Act.

Is your workshop looking at all that the institutional arrangements might bring this parallel engineering development together?

Dr. WOODSON. Not really, Frank. We really did concentrate on trying to assess how well the engineering development needs can now be defined and also how industry might get involved in terms of a number of different modes.

One of those, of course, might be through a Center for Fusion Engineering, but we did not specifically discuss whether or not there ought to be a Center for Fusion Engineering.

Dr. WATSON. You mentioned that a view was expressed that the primary objective of the program should not be to produce electric generating facilities. What then should be the objective of the program? To produce neutrons and worry later to do with them?

Dr. WOODSON. Once again there were a number of people in attendance and a number of different views expressed.

If I can sort of paraphrase them all, I think at least there was the very definite opinion expressed by one of the people in attendance—not me—that if you look at potential applications of fusion, it appeared to him that the easier thing to do was to make fusion for the purpose of making neutrons and then do something with the neutrons. Then, the next hardest was a hybrid fusion machine and then the hardest of the three was to make a pure-fusion power electric generating plant that would be commercially competitive.

So at least that was a view expressed by one of the attendees in terms of how easy it was to get from here to there.

I might inject a personal view at the moment. From looking at conceptual reactor designs that have been done in terms of what we now know and what we expect to know in the immediate future, it is my opinion that a DT based electric generating fusion plant won't come close to being economically attractive compared to the alternatives for producing electricity.

That is not to say that it never will be, but it seems to me we need to learn a whole lot more about the science underlying fusion and we need to do a lot more technological development before we conclude that that power fusion electric generating plant based on a DT fuel cycle is really what we want. Maybe we want to look more closely at advanced fuel cycles where we don't have to breed and handle tritium and where the energy comes out not as 14 MeV neutrons, but lower energy neutrons and more charged particles. That would change all of the technological problems associated with fusion power.

I was visiting with John during the intermission and we were observing that it is oftentimes instructive to go back and read the predictions and technical literature about 30 years ago and see how accurately people who were writing then predicted what was actually going to happen. And it turns out that their predictions were by and large wrong and we should be humbled as we predict the future.

Mr. RICHARDSON. I might amplify what you said in response to Harlan's question. There are alternative functions for fusion reactors; namely, simply producing neutrons for whatever use, or producing them for the purpose of breeding fission fuel or ultimately

producing full-scale electric generation. These objectives were advanced, I think, in response to a felt need for interim successes to sustain a long-term highly expensive program. That was one of the things that was brought out at the workshop—what shall be put forward for our interim goals and interim successes to sustain public and political interest.

In reaching for an answer to these questions, these goals were suggested.

Mr. REICHLE. I have a question and I don't know whether it is for Herb or John Clarke. But in the discussions so far, it is being recognized that we developed a program a couple years ago, which had an up curve, now we are in a different world situation, and we are on a relative flat budget. John Clarke has developed a very effective plan adapted to realities of today. But did your workshop or have you, John, considered, the alternate plan? The world changes. We have seen in the past in a short period of time things are very different from what they were 2 years ago. We started to build a Shippingport craft reactor and when we discovered the British were ahead, we called a halt. We went 180 degrees and changed it to a power producer.

Suppose the Soviets make a significant breakthrough, or suppose we have significant energy crisis again, along the lines Steve Dean has been worrying about. Do we know what we would do alternately to what the present plans provide? How we can really effectively ramp up a fusion program to make it an earlier achievable program or more effective program with more dollars? Suppose we had more dollars per year for the next 5 years after 1 year from now or 2 years from now? Do we have in our back pocket, or have we given thought to what would we do if more funding were made by OMB or the Congress?

Dr. CLARKE. Prof. Woodson, don't you have the answer to that?

Mr. REICHLE. It is clearly yours, John.

Dr. CLARKE. It seems to me, Len, what you are asking is whether or not we have what Ed Kintner used to refer to as the Shliefer plan. I think the general answer is no but let me qualify that. It seems to me there are two things that one would have to consider in responding to changed circumstances. One is the technical development of the program. The level of technical development is fundamental because if you don't have the technical capability to move ahead, you are dead in the water.

The second thing, of course, is the organizational arrangements. My answer to the question saying we don't have a plan really refers to the latter, the organizational arrangement.

The plan that we have laid out for the fusion program tries to address all of the technical issues and not leave anything undone. We will have engineering designs for both the tandem mirror and the Tolomak sufficient to move ahead. We even have a Tokomak design that could be built today that would produce power. So if the situation changes tomorrow, we will have the wherewithal to move ahead technically. Furthermore, we plan to keep updating the designs in these two areas.

As of today, we would have to move with the Tokomak. Three or four years from now, the mirror would have developed to the point

where it may well be competitive and we would have the wherewithal to move ahead with that.

The organizational questions, I think, are more difficult. What is the type of appropriate organizational arrangement? Is it the Center for Fusion Engineering as proposed in the past? Is it a NASA type arrangement? Personally, I think, that would have to be determined by circumstances at the time, so we are not putting much effort into that detailed planning.

Dr. WOODSON. If I could comment, I would remind you again that our workshop was a very small exercise and we did not address budgetary issues and things of that sort. I would say that there were such views expressed in the workshop. Once again they were not expressed by me, but by some attendees. That was that fusion is very important to the long-term future and it's a long-term option.

But to the extent that the fission option stays on track and we get the breeder, then there is no real urgency to develop fusion and one can carry out an ordinary step-by-step program. If for some reason the breeder program gets untracked, then there is an urgency to fusion and you really would need to begin to telescope things and move very, very rapidly ahead. So the urgency with which you need fusion is very much tied to what happens in the fission program. There was a bit of discussion of that sort of thing in our workshop.

Mr. RICHARDSON. I think we ought to move along now to hear from Ron Davidson. Ron is professor of physics at MIT and also director of the Plasma Fusion Center at MIT. As everyone knows, he is also Chairman of the Magnetic Fusion Advisory Committee to the Department of Energy. Ron tells me that there are some limitations on how far he can speak in this capacity and I will let him explain that further to you.

STATEMENT OF DR. RONALD C. DAVIDSON, CHAIRMAN OF THE MAGNETIC FUSION ADVISORY COMMITTEE TO THE DEPARTMENT OF ENERGY, PLASMA FUSION CENTER, MASSACHUSETTS INSTITUTE OF TECHNOLOGY

Dr. DAVIDSON. I would like to thank you for the opportunity to address this group. I will not discuss the Magnetic Fusion Advisory Committee recommendations. As you know, MFAC receive its charge from the Department of Energy and in order to summarize findings and the like, it is necessary that I be accompanied by Dr. Trivelpiece as a representative of DOE.

The second point is that our deliberations are in progress with regard to the original charges by DOE. We expect the recommendations to be finalized in the next few months and I would be glad to summarize findings for you in January after the next meeting of MFAC.

I might indicate as general background that the three areas of charges by DOE are the following. We have received these charges 3 months ago. The first area relates to an evaluation of the technical status program plans and development base for the mainline tandem mirror and Tokamak confinement approaches.

The second area relates to evaluation of the technical status, plans and development of the backup Bumpy Torus, reversed field, pinch, and Stellerator concepts, and the third area relates to an evaluation and making of recommendations regarding upgrade options for TFTR.

We have established three panels in these areas. Two of the panels have reported back at our August 30–31 meeting. As I indicated, we are in the process now of formulating our recommendations related to these charges.

I would like to take a few minutes to summarize for you my own findings in two important areas. The first area relates to recent technical advances which have, in my opinion, been very significant, but perhaps somewhat overlooked in the present budget climate.

The second area relates to some comments and observations I have with regard to international cooperation.

These represent my own views, although I believe they are shared by a majority of the Fusion Committee.

IMPORTANT TECHNICAL ISSUES FOR MAGNETIC FUSION

- SCIENTIFIC
- ENGINEERING AND TECHNOLOGY

SLIDE 1

There are two issues related to magnetic fusion energy development (slide 1). Broadly speaking they are: scientific issues, the first category, and engineering and technology issues. Under scientific issues, I have listed here three main items. First of all it is necessary to demonstrate in the magnetic confinement configurations stable operation at moderate values of beta, where beta is the ratio of plasma pressure to magnetic pressure.

SCIENTIFIC ISSUES

- STABLE OPERATION AT MODERATE BETA

$$\text{BETA} = \frac{\text{PLASMA PRESSURE}}{\text{MAGNETIC PRESSURE}}$$

- MAINTAIN GOOD CONFINEMENT AT HIGH $N\tau_E$

$$N\tau_E \gtrsim 10^{14}\ \text{CM}^{-3}\text{-SEC}$$

- HEAT PLASMA TO HIGH TEMPERATURES

$$T \sim 100 - 150\ \text{MILLION}\ ^{o}\text{C}$$

SLIDE 2

Second, you would like to maintain good confinement properties at high values of the Lawson parameter n tau, where n is the fuel density (slide 2). Particularly, you like that product to be in the neighborhood of, or exceeding 10 to the 14th per cubic centimeter seconds.

In the third area, you like to heat and sustain the plasma fuel to high temperatures in the range of 100 to 150 million degrees centigrade.

ENGINEERING AND TECHNOLOGY ISSUES

- HEATING SYSTEMS
- MAGNET SYSTEMS
- NUCLEAR SYSTEMS/BLANKETS AND SHIELDING
- MATERIALS

SLIDE 3

Engineering and technology issues have been touched on by the previous speakers (slide 3). I mentioned four, Dr. Clarke also included vacuum systems. I have listed here heating systems, magnetic systems, nuclear systems, related to blankets and shielding; and finally I have listed materials.

The first two areas, as indicated by Dr. Clarke, to a large extent are being addressed in significant fashion by the existing confinement facilities that are being operated or are under construction, such as TFTR and MFTF.

THERE HAS BEEN SIGNIFICANT RECENT TECHNICAL PROGRESS

IN THE SCIENTIFIC DATA BASE AND TECHNOLOGICAL

PROSPECTS FOR MAGNETIC FUSION ENERGY

SLIDE 4

This viewgraph (slide 4) shows the main message I would like to leave you with in the first part of my presentation. In the last 2 years since the 1980 Engineering Fusion Act, in fact there has been

very significant expansion in the data for magnetic fusion energy. I would like to go through the various confinement approaches and give you an outline of what some of the specific areas of technical achievement have been.

TOKAMAKS

- STABLE OPERATION HAS BEEN ACHIEVED IN THE ISX-B, PDX, AND D-III TOKAMAKS DOWN TO LOW MHD SAFETY FACTORS ($q < 2$). THIS TECHNIQUE HAS ALLOWED CONFINEMENT SUTDIES TO BE EXTENDED INTO THE AVERAGE STABLE BETA RANGE ABOVE 3%. THE PRESENT OUTLOOK IS FAVORABLE TO ACHIEVING TOKAMAK REACTOR OBJECTIVES IN THE 5-10% BETA RANGE.

- RF HEATING AT THE ION AND ELECTRON CYCLOTRON FREQUENCIES HAS GENERATED MULTI-keV ION AND ELECTRON TEMPERATURE INCREASES IN THE PLT AND T-10 TOKAMAKS, RESPECTIVELY. THE MEASURED EFFICIENCIES ARE HIGH AND POINT TO ECONOMICALLY ATTRACTIVE REACTOR HEATING SYSTEMS.

- RF CURRENT DRIVE IN THE LOWER-HYBRID FREQUENCY RANGE HAS BEEN USED TO SUSTAIN TOKAMAK CURRENTS OF SEVERAL HUNDRED KILOAMPERES FOR SEVERAL SECONDS IN PLT PLASMAS OF $10^{13}cm^{-3}$ PEAK DENSITY, AND FOR SHORTER TIMES IN ALCATOR C PLASMAS OF $10^{14}cm^{-3}$ PEAK DENSITY. THE LOW-DENSITY RESULTS HELP INSURE THE TECHNICAL FEASIBILITY OF THE 10^4 SECOND BURN TIME DESIRED FOR AN ECONOMICAL TOKAMAK REACTOR. THE HIGH-DENSITY RESULTS OFFER THE PROMISE OF TRUE STEADY-STATE TOKAMAK OPERATION AT REACTOR-LEVEL DENSITIES.

SLIDE 5

Now, with regard to specific technical achievements for Tokamaks the first item in the slide (slide 5) indicates that in the area of plasma beta, three devices, ISX-B, PDX, and D-III have achieved beta is in excess of 3 percent, which is significant progress relative to a few years ago when the beta was approximately 1 percent.

In addition, at the Baltimore meeting, it was reported that this average beta has been increased to 4.7 percent on the Doublet III device. So in the area of beta, there has been significant progress.

The second item relates to the area of RF heating, that is to say intense radiation, injected into the plasma in an attempt to increase plasma temperature. Both with regard to low frequency

heating and high frequency heating, there has been multikiloelectron volt increases in temperature using RF heating. Multikiloelectron volt means multitens of millions of degrees. In relation to the third item, it was mentioned by Dr. Clarke that there are prospects for the Tokamak approach to be developed into a steady-state device through use of RF to drive the toroidal current. This particular item on the viewgraph indicates that in the PLT Tokamak, there has been RF current drive demonstrated in plasma of peak density of 10 to 13th per cubic centimeter.

In Alcator C, at 10 to the 14th peak density, the results help insure technology feasibility of 10 to the 4th second burn time desired for the Tokamak. The high density results offer the promise of true steady-state Tokamak operation at reactor density.

TANDEM MIRRORS

- END PLUGGING BY TANDEM MIRROR ELECTROSTATIC POTENTIALS HAS BEEN CONCLUSIVELY DEMONSTRATED ON TMX WITH $N\tau_p \simeq 10^{11} CM^{-3}$-SEC IN THE CENTRAL SOLENOID AT TEMPERATURES $T_i \sim T_e \sim 100$ EV, REPRESENTING TWO ORDERS-OF-MAGNITUDE IMPROVEMENT IN CONFINEMENT RELATIVE TO A "SINGLE" MIRROR AT THE SAME TEMPERATURE.

- HIGH-BETA MHD STABILITY IN THE CENTRAL SOLENOID HAS BEEN DEMONSTRATED WITH NEUTRAL BEAM HEATING IN TMX (UP TO 20% AVERAGE BETA WITH ANISOTROPIC PRESSURE, AND 40% MAXIMUM BETA ON AXIS) AND WITH ION CYCLOTRON RF HEATING IN PHAEDRUS. EXPERIMENTS TO TEST MAJOR ADVANCES IN REACTOR END-PLUG DESIGN HAVE BEEN INITIATED ("AXICELL" MAGNET GEOMETRY ON TARA AND "THERMAL BARRIERS" ON TMX-UPGRADE).

- MFTF-B CONSTRUCTION HAS PASSED THE MIDWAY POINT WITH SUCCESSFUL TESTS OF THE FIRST SUPERCONDUCTING YIN-YANG (END-PLUG) MAGNET AT FULL DESIGN FIELD (7.7 TESLA AT THE NbTi CONDUCTOR).

SLIDE 6

This slide (slide 6) summarizes the areas of significant technical progress in the tandem mirror area. The first item indicates that the basic tandem mirror concept has been conclusively demonstrated on the TMX experiments at Livermore with the product of density times particle confinement time of approximately 10 to the 11 per cubic centimeter-seconds at temperature at around 100 electron volts. This represents two orders of magnitude improvement in confinement relative to a single mirror at the same temperature.

There is also important progress made in the area of achieving high beta MHD stability both in the TMX experiment and in Phaedrus at the University of Wisconsin.

The second item in this slide also shows that experiments to the test major advances in reactor end plug design have been initiated. These include the axicell magnet geometry on TARA and thermal barriers on TMX upgrade.

Here is an indication of how a large confinement facility such as MFTFB construction, has passed the midway point with successful test of the first super conducting Yin-Yang magnet at full design field, 7.7 Tesla at the conductor.

The Tokamak and mirror concepts account for approximately, according to my numbers at least, 85 percent of the confinement resources that the Department of Energy expends in magnet fusion.

In addition to these mainline programs, about 15 percent of the resources are spent on the development of backup concepts, advanced concepts, alternate concepts, you hear them designated in various ways. These are the bumpy torus, reversed field pinch, and the stellarator approaches. The stellarator approach is being explored in the United States at a rather modest study level.

REVERSED FIELD PINCH

- CONFINEMENT EXPERIMENTS CARRIED OUT ON THE ZT-40 REVERSED FIELD PINCH HAVE PRODUCED PLASMA PARAMETERS THAT CONSIDERABLY EXCEED ORIGINAL EXPECTATIONS. ZT-40 IS NOW OPERATING AT REACTOR-RELEVANT BETA LEVELS UP TO 10-15 PERCENT FOR 20 MILLISECONDS, AND THE DISCOVERY OF A "DYNAMO" EFFECT MAY SIMPLIFY THE REACTOR DEVELOPMENT OF THE REVERSED FIELD PINCH BY PROVIDING A MECHANISM FOR SUSTAINING THE REVERSED FIELD CONFIGURATION AND ALSO PROVIDING A POTENTIAL MECHANISM FOR STEADY-STATE CURRENT DRIVE.

- ADVANCES IN THE DEVELOPMENT OF REVERSED FIELD PINCH REACTOR MODELS HAVE ALTERED THE NATURE OF REACTOR OPTIONS AVAILABLE TO THE MAGNETIC FUSION PROGRAM. THE RESISTIVE COPPER MAGNET COMPACT-REVERSED-FIELD-PINCH-REACTOR (CRFPR) WITH ITS DT AND DD FUEL OPTIONS OFFERS THE PROSPECT OF AN END PRODUCT THAT IS QUALITATIVELY DIFFERENT FROM CONVENTIONAL REACTORS IN REDUCING THE SIZE, MASS AND COMPLEXITY OF REACTOR SYSTEMS.

- INTERLABORATORY COOPERATION HAS LED TO MAJOR ADVANCES IN THE ABILITY TO PRODUCE COMPACT TORUS CONFIGURATIONS WHICH COMBINE THE BEST FEATURES OF THE TOROIDAL AND LINEAR APPROACHES. THE EQUILIBRIUM AND STABILITY PROPERTIES OF THESE CONFIGURATIONS ARE NOW BEING STUDIED IN EXISTING LABORATORY EXPERIMENTS.

SLIDE 7

Next slide, please (slide 7). All of these concepts play a very important role in the development of new ideas. I believe this was indicated earlier with regard to potential reactor advances, whether it is steady-state in EBT, or the desirability of high beta as evidenced in the reversed field pinch concepts. All of the backup concepts play an important role in looking at the technical issues related to features which make the reactor manifestation of the concept more desirable, and in turn exert some pressure back on the mainline concept reactor designs.

Confinement experiments carried out in the ZT-40 reversed field pinch have produced plasma parameters exceeding original expectations. It is now operating at beta levels up to 10 to 15 percent for 20 milliseconds. The reversed field pinch approach has undergone significant modification with regard to reactor design. It now ap-

pears that the reversed field pinch offers the possibility of a compact moderately high power density reactor.

ELMO BUMPY TORUS

- NEOCLASSICAL ELECTRON TRANSPORT SCALING HAS BEEN VERIFIED IN 200kW, 28GHz OPERATION OF THE EBT-S ELMO BUMPY TORUS EXPERIMENT. THIS GREATLY INCREASES CONFIDENCE IN PROJECTIONS FOR BOTH EBT-P AND THE ELMO BUMPY TORUS REACTOR, WHICH ARE BASED ON A NEOCLASSICAL TRANSPORT MODEL.

- IMPORTANT DEVELOPMENTS IN COUPLED RING-CORE STABILITY THEORY, AND SELF-CONSISTENT APPLICATION OF THE ELMO BUMPY TORUS REACTOR MODEL BASED ON THIS THEORY PREDICT ATTRACTIVE REACTOR PERFORMANCE WITH A TOTAL RECIRCULATING POWER FRACTION OF ABOUT 10%.

- FAST WAVE ION CYCLOTRON HEATING EXPERIMENTS ON EBT-S SHOW EFFICIENT (OVER 40%) RF POWER COUPLING AND ION HEATING. THESE EXPERIMENTS PROVIDE AN IMPORTANT PHYSICS AND TECHNOLOGY BASE FOR TESTING ION CONFINEMENT AND STABILITY ON EBT-P.

SLIDE 8

There has also been significant progress in the Elmo Bumpy Torus area (slide 8). Let me select one item from this viewgraph. I will focus on the first one. The bumpy torus concept both with regard to the reactor projections and also with regard to projections for future EBT facilities relies very heavily on having neoclassical electron transport. The EBT-S facility at the Oak Ridge National Laboratory has carried out an extensive set of experiments which indicates over a large parameter range in that facility that neoclassical electron scaling pertains. This gives encouragement, of course, with regard to both the EBT reactor design and future steps in the experimental program.

STELLARATORS

- NEUTRAL-INJECTION EXPERIMENTS IN THE WENDELSTEIN VII A STELLARATOR (FEDERAL REPUBLIC OF GERMANY) HAVE ACHIEVED $N\tau_E \simeq 3 \times 10^{12} cm^{-3} SEC$ AT ION TEMPERATURES $T_i \simeq 1 keV$, CORRESPONDING TO VALUES OF THE PRODUCT $(n\ E) \times T_i$ SURPASSING ALL NON-TOKAMAK DEVICES.

- CURRENT-FREE STELLARATOR OPERATION HAS BEEN ACHIEVED BOTH IN NEUTRAL-INJECTION EXPERIMENTS ON W VIIA AND IN THE AFTERGLOW OF ELECTRON CYCLOTRON HEATING ON HELIOTRON E (JAPAN). IN BOTH CASES, ELECTRON ENERGY CONFINEMENT IN THE CURRENT-FREE REGIME IS SUBSTANTIALLY IMPROVED RELATIVE TO TOKAMAKS.

- MAGNETICALLY-TRAPPED ENERGETIC BEAM IONS, ARISING FROM PERPENDICULAR INJECTION, ARE WELL CONFINED IN W VIIA, APPARENTLY DUE TO THE SELF-CONSISTENT RADIAL ELECTRIC FIELD. COMPUTATIONAL RESULTS INCLUDING SUCH FIELDS ARE IN AGREEMENT WITH THE OBSERVATIONS, AND PREDICT THAT ION ENERGY CONFINEMENT SHOULD BE ACCEPTABLE IN A STELLARATOR REACTOR. IN W VIIA, ION THERMAL TRANSPORT IS CONSISTENT WITH NEOCLASSICAL PREDICTIONS.

SLIDE 9

The final area in the area of backup concepts relates to stellarators (slide 9). This is an instance where we have virtually no experimental activity in the United States, but a very strong experimental activity abroad. If I look at the first item in this viewgraph, neutral-beam injection experiments in the Wendelstein VII A Stellarator in the Federal Republic of Germnay have achieved τ values of approximately 3 times 10 to the 12 per cubic centimeter seconds and ion temperatures in the kilovolt range corresponding to values of the product τ, times by ion temperature surpassing all non-Tokomak devices. This is an example of a concept being explored vigorously in Japan, Europe, and also the Soviet Union.

The message that I would like to leave you with with regard to these first several viewgraphs is that indeed in the past 2 years there has been a significant amount of technical progress in fusion. As I said earlier, the present budget climate forces us to focus on items such as program planning. In doing this, we should recognize that the fusion program technically is very healthy at the present time, and indeed, I believe it is ready to move ahead at as early a date as possible.

I would like to make a few comments in the area of international cooperation. The fusion program in the United States is significant-

ly benefiting from research in other nations. Moreover, fusion research and development has traditionally been characterized by active international cooperation in exchange of ideas and information that has led to rapid technical progress.

The most striking example of this is the rapid U.S. and worldwide development of the Tokamak concept originally investigated in the Soviet Union. I should say that other nations' fusion programs also benefit from research activities and concepts developed in the United States.

For example, the stellarator approach originally developed in the United States is now actively pursued in Europe, the Soviet Union, and Japan. International cooperation in fusion takes place not only at the level of exchange of ideas and information. The joint United States-Japan research program on the Doublet-III Tokamak is an excellent example of a productive international cooperation on a major U.S. research facility.

INTERNATIONAL COOPERATION

- THE U.S. FUSION PROGRAM AND THE DEVELOPMENT OF FUSION ON A WORLDWIDE BASIS HAS SIGNIFICANTLY BENEFITED FROM THE ACTIVE EXCHANGE OF INFORMATION AND IDEAS. INTERNATIONAL COOPERATION IN FUSION RESEARCH SHOULD CONTINUE TO RECEIVE STRONG EMPHASIS IN THE U.S. PROGRAM.

- TO A LARGE EXTENT, THE DESIGN OF FUSION FACILITIES AND EVOLUTION OF PROGRAM PLANS ON A WORLDWIDE BASIS HAVE PLACED EMPHASIS ON INVESTIGATIONS OF COMPLEMENTARY TECHNICAL ISSUES. THIS MINIMIZATION OF DUPLICATION IS BOTH COST-EFFECTIVE AND CONDUCIVE TO RAPID TECHNICAL DEVELOPMENT. FOR EXAMPLE, LARGER STEPS CAN BE TAKEN WITHIN EXISTING BUDGETARY CONSTRAINTS.

SLIDE 10

On this viewgraph (slide 10) I indicate two of my own findings with regard to international collaboration. The first is a statement that the U.S. fusion program in development of fusion on a worldwide basis has significantly benefited from the active exchange of information and ideas. I believe that international cooperation in fusion research should continue to receive strong emphasis in the U.S. program.

Second, to a large extent the design of fusion facilities and evolution of program plans on a worldwide basis have placed emphasis on investigation of complementary technical issues. This minimization of duplication is both cost-effective and conducive to rapid technical development.

For example, larger steps can be taken within existing budgetary constraints.

For each fusion approach that is being pursued in the world, it is my view that the U.S. effort is largely complementary to activities of other nations.

INTERNATIONAL FUSION EFFORT

	U.S. EFFORT	EFFORT IN OTHER NATIONS
TOKAMAK	MAJOR	MAJOR (EUROPE, JAPAN, SOVIET UNION)
TANDEM MIRROR	MAJOR	MODERATE (JAPAN, SOVIET UNION)
ELMO BUMPY TORUS	MODERATE	MODEST (JAPAN)
REVERSED FIELD PINCH	MODERATE	MODERATE (EUROPE, JAPAN)
STELLARATOR	VERY MODEST (MOSTLY STUDY ACTIVITIES)	MODERATE (EUROPE, JAPAN, SOVIET UNION)

SLIDE 11

This slide (slide 11) indicates a breakdown of the international fusion effort. We have a major effort in Tokamaks, and tandem mirrors. Europe, Japan, and the Soviets have major efforts in Tokamaks. There is an effort in tandem mirrors in Japan and the Soviet Union and technology work being done in the Federal Republic of Germany. With regard to the Elmo Bumpy Torus, the U.S. effort is moderate, and there is a modest amount of research going on in Japan. The reversed filled pinch effort in the United States and abroad, I would characterize as moderate.

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The stellarator effort is quite modest in the United States and consists mostly of study activities. There are moderate stellarator programs abroad in Europe, Japan, and the Soviet Union.

That completes what I have to say.

Mr. RICHARDSON. Thank you very much. Any comments for Ron on this point?

Dr. WOODSON. Wasn't the stellarator invented in this country?

Dr. DAVIDSON. It was. But it is not being neglected.

Mr. RICHARDSON. Anyone else?

I think we can invite any comments from the observers as well as the panel itself around the table if there are any such comments.

VOICE. On the stellarator approach, do you have a study going on that?

Dr. DAVIDSON. The stellarator approach is toroidal, like the Tokamak. It makes use of not simple toroidal field coils, but external helical windings to produce the magnetic field geometry. There are study programs at Oak Ridge, the Princeton Plasma Physics Laboratory and other locations, which are looking at advanced versions of the stellarator concept and because of the great progress abroad, I think it is accurate to indicate that some of these institutions are giving very serious consideration to proposing stellarator facilities in the U.S. program.

Mr. REICHLE. Ron, has your committee taken a look at Bob Bussard's reactor, or other approaches, other than what you identified?

Dr. DAVIDSON. The technical information from OHTE when it is operated as a reversed field pinch, is being considered by panel No. 2 in looking at the evaluation of alternate concepts.

In terms of looking at the RIGGATRON approach, I don't believe that any of the MFAC panels have looked at that in detail. I would say, in terms of TFTR upgrade options, that there is certainly consideration being given to a high-field ignition experiment as a possible upgrade option, but I don't believe that it is of the highly compact variety like the RIGGATRON.

Mr. REICHLE. Thank you.

Mr. GRAHAM. One question. Earlier I understood your committee had considered a fourth panel to look at the engineering technology and you didn't mention it today. I wonder what happened to it?

Dr. DAVIDSON. At the first meeting of the MFAC, there was a strong sentiment of the committee that it was important to establish a fourth panel to look at engineering and technology issues related primarily to ETR.

In our second meeting, we discussed formation of such a panel and it was decided that we would await further charges from the DOE before establishing such a group. I think there is a fairly clear indication that if you look at the analysis that the first panel has done in looking at tandem mirrors and Tokamak, a number of ETR related issues have been addressed by that panel.

Second, in looking at TFTR upgrade options, panel 3 is also coming to grips with the issues that panel 4 would have addressed.

Mr. RICHARDSON. Well, Ron, if I could venture to summarize, and tell me if I am wrong. It seems to me that you said the science is in hand with fair certainty. You have left much unsaid about engineering. I would presume that one may run into formidable engi-

neering problems. You have also left much unsaid about cost to overcome these engineering problems.

But if we have a mind to, we could proceed to the engineering with confidence in the science.

Dr. DAVIDSON. I anticipated the previous speaker would focus on the engineering issues, so I emphasize primarily the scientific ones.

I would not make such a strong statement as to say that the science is in hand. I simply wanted to indicate that on all fronts, in fact, for all concepts, we are looking at a situation where there has been significant technical progress. This is indeed that very encouraging. If the scientific data base was there 2 years ago for the Magnetic Fusion Engineering Act, for example, it is certainly there today in a much more encouraging way.

Mr. RICHARDSON. Thank you very much, Ron, for that survey.

We now want to turn to the views of the utilities and the industry. I am not at all sure whether the next item is a duet or two solos. You may enlighten me on this point, Bob.

Dr. SCOTT. I can't speak for Frank.

Mr. GRAHAM. We are two solos, John.

Mr. RICHARDSON. Then we will turn to the two soloists.

The suggestion has been made, Bob, to move to a mike. While you are doing that I would say that Bob Scott has had an extremely long career in fusion, beginning, coincidentally, with the beginning of the program at Los Alamos in the 1950's. On a personal note, I had some tangential contact with the program about then, and then went on to other things after that.

After Bob's experience with Los Alamos, in the 1950's, he was with General Atomic for about 10 years; then at the University of Tennessee as professor, and as consultant for Oak Ridge National Laboratory. He moved to Washington where he managed the mirror program for the Department of Energy, and to the Electric Power Research Institute in 1975.

Bob.

STATEMENT OF DR. F. R. SCOTT, PROGRAM MANAGER, THE EPRI FUSION POWER SYSTEMS PROGRAM

Dr. SCOTT. It is indeed a great pleasure to be here to talk to you about our Electric Power Research Institute fusion program. I probably should give you a little bit of a prelude to it.

The institute itself is composed of 700 people. It is a nonprofit institute funded by the utility industry in this country. Its total budget in terms of R&D is about $250 million a year. There are 42 separate programs of which the fusion program is one. These span all of the energy issues of the utility industry.

The fusion program itself is within the advanced power systems division, which has primary responsibility for liqufication and gasification of coal. You might wonder why fusion is there rather than in nuclear. It is a very advanced program. It is aimed toward something in the future, and Advanced Power Systems Division engineering issues seem to match the fusion program more effectively than the somewhat more near-term problems that the nuclear division has at EPRI.

Our fusion program since 1974 has had 41 projects with a total expenditure of about $28 million. Our 1982 budget is $2 million. As Ed Kintner said some several years ago we know how to stretch the dollar, as you will probably see from some of my presentation here. Our ultimate objective of course is to identify and encourage the development of fusion power systems that are of use and value to the utility industry.

THE EPRI FUSION POWER SYSTEMS PROGRAM

F. R. Scott
Program Manager

PROGRAM OBJECTIVES:

- INDUSTRIAL INVOLVEMENT
- PRESENT MAJOR ACTIVITIES

SLIDE 1

However, we have five near-term goals (slide 2). I might say EPRI has the same kinds of management issues that the Federal program does. We have 5-year plans just like they do. Each year we do them over again. And each year we decide on what our projects and activities will be for the next year. What I am going to do now is indicate the objectives of the program.

By the way, I should mention, obviously, I am the program manager. But we have two technical project managers, Dr. Noel Amherd, who handles the analysis and technical development of the program, and Dr. Ken Billman who handles the inertial confinement part of the program. EPRI has attempted to keep a reasonable balance between magnetic and inertial fusion. We have projects in inertial as well as magnetic. The five program objectives are listed in the second slide.

FUSION POWER SYSTEMS PROGRAM

FIVE MAJOR OBJECTIVES:

- Identify and evaluate technological issues essential for the engineering development of practical fusion power systems for the utility industry.
- Encourage innovative approaches to the engineering development of fusion power.
- Maintain state-of-the-art understanding for the utility industry.
- Conduct engineering project activities that are generic to fusion.
- Maintain close liaison with DOE and define appropriate engineering experiments which will complement the DOE experimental program.

SLIDE 2

First we try to identify and evaluate technical issues essential for the engineering development of the program from the viewpoint of the utilities (slide 2). We certainly work very closely with the national program, but obviously, we have a perspective that is quite different than the Federal program.

For example, some 7 or 8 years ago, we funded a $2.1 million design for the Doublet device which was built by the Federal Government, and we are very pleased with the Doublet III performance. We feel like it was our baby, although it isn't really, because the Federal Government certainly built it and operated it.

We also encourage innovative approaches in engineering development. For example, some 4 years ago we funded a theoretical study to develop steady state drives for Tokamaks. We recognized at that time that it was an important innovation that could improve the performance of Tokamaks. Third, of course, we maintain the state-of-the-art understanding for the utilities. We attempt to transmit to the utilities the technical progress of the national and international program.

At the moment we was preparing a 150-page document written for the utilities on the present state-of-the-art of magnetic and inertial fusion. The document should be finished by January. It will put the programs' technical progress, present state-of-the-art, and the promises of the overall fusion program in a language the utilities can read.

Fourth, we conduct engineering project activities that are generic to the fusion program. We feel very pleased to participate in the TFTR project. I will discuss that in a minute on a specific project. We try to find activities where EPRI can participate with the national program that are effective for us and within our budget and our objectives. This means we are not very involved in the scientific progress, although we keep track of it. We are more

interested in identifying engineering activities in which we may be able to participate.

Finally, of course, we want to maintain close liaison with the Department of Energy where all of the major technical activities are now occurring, and find appropriate engineering experiments that we may be able to participate in that are obviously complementary to the Federal program.

INDUSTRIAL INVOLVEMENT IN EPRI FUSION PROGRAM

McDONNELL DOUGLAS

BABCOCK & WILCOX

COMBUSTION ENGINEERING

WESTINGHOUSE

TRW

BECHTEL

PUBLIC SERVICE ELECTRIC & GAS

NORTHEAST UTILITIES

ONTARIO-HYDRO

BURNS & ROE

PACIFIC GAS & ELECTRIC

BOEING

MATHEMATICAL SCIENCES NORTHWEST

GENERAL ATOMIC

GENERAL ELECTRIC

SLIDE 3

The next slide (slide 3) is something I hope is of interest to the committees. I thought it might be useful for me to discuss the details to which we go and how we function with respect to our con-

tractors. EPRI is a management shell. We don't do any research ourselves. We contract it out. This is a list of some of our contractors. I would like to comment on a few things with respect to these contractors.

McDonnell Douglas became involved with the fusion program through a project at EPRI on the systems analysis of the first wall and blankets. We were very pleased to involve an aerospace systems company in the program. We felt, and this was some 4 or 5 years ago, that the aerospace industry had something to bring to the table in fusion. This certainly has proved to be the case.

Combustion Engineering recently has completed a study for us examining the designs of fusion-fission blanket modules from the viewpoint of thermal mechanical stress, materials problems, and neutronics. They were to look at three designs and comment on their engineering realism.

Westinghouse also has been involved with us in many ways. Westinghouse and TRW are doing a comparative study on the possible differences in development plans between fusion electric and fusion hybrids. People say hybrids are a lot easier to develop than fusion electric. We want to document this and see really what the development differences are. This project is in conjunction with a hybrid project that I will discuss a little later. You will notice that some utilities are actually contractors of ours, doing technical work for us at EPRI. We try to use contractors in their area of expertise. We feel that private industrial companies have a very important contribution to make to fusion development. You might wonder why they get involved. We went to Combustion Engineering last year before we contracted with them and talked to three or four levels of management, we asked them "why are you interested in participating in the fusion program?"

Their answer involves something that hasn't yet been mentioned here this morning. Fusion pushes the state-of-the-art in many technologies: high-voltage switches, power systems, power transfer systems, vacuum systems, and superconducting magnets. In so doing, it is of value to the private sector to be involved in these technologies to see how they may either participate in them in the fusion program or in other areas. They find their participation even at a modest level is of value to the corporation itself. It prepares their readiness. It also helps them keep abreast of future developments that might have effect on the economy.

Utility companies, some 39 of them representing 239 gigawatts of producing power in this country, or about 40 percent of the total power producing utilities in the country, have some kind of involvement with the fusion program. Most of it is independent of EPRI and most of it is independent of DOE.

EPRI at every level of management has utility advisory committee. Fusion has one and there are 12 utilities represented. There are 27 utilities represented on university advisory committees, such as the University of Wisconsin and Princeton. The AIF Fusion Committee has seven utilities, and the Fusion Power Associates, Steve Dean's organization, has 11 utilities represented.

In terms of project support, utilities actually have personnel in the laboratory. There are people from the utilities that spend some of their time working in national laboratories. There are 25 utili-

ties in the country that send money to the University of Texas, University of Rochester, and even to Princeton. There is a small fund at Princeton coming from the utilities. They are involved and you might ask why.

Utilities feel the fusion program is a possible future energy system. But they remember the past. They feel that they weren't involved in the nuclear industry early enough to have something to say about what has happened in the nuclear industry. Because of that experience they feel they want to have a say in what is done in the early development of fusion.

PRESENT MAJOR ACTIVITIES

- Evaluating the fusion–fission hybrid option
- Examining engineering opportunities on existing and planned fusion facilities
- Conducting a technical risk assessment on fusion fuel cycle
- Continuing the design and construction of a tritium breeding blanket module on TFTR
- Conducting a technical risk assessment on the inertial fusion system directed toward power production
- Characterizing an engineering test facility for light ion inertial fusion

SLIDE 4

In order to give a picture of the present major activities of the EPRI program I plan to go through this fast (slide 4). But a couple of things I want to stress. No. 1, EPRI is at the present time involved in early engineering evaluations. We have over the last 2 years gone through a change in our program. We are emphasizing engineering. What kinds of engineering issues need to be addressed by EPRI for the utilities? One of the big issues in our program has been fusion-fission hybrids. EPRI in the last 5 years has spent 40 percent of the total amount of money in this country on fusion-fission hybrid studies. We have been driven to this by our utility advisers that say to us, "we want to be sure that our $300 billion investment in light-water reactors by the year 2000 is protected. We want to be sure there is enough fuel at a reasonable price in the future."

UNRESOLVED ISSUES OF HYBRIDS AS FUEL FACTORIES

1) Need

2) Alternatives

3) Fuel Resources

4) Fissile Fuel Cycle —
Reprocessing and Waste

5) Comparison of development needs for fusion electric vs fusion–fission hybrids

SLIDE 5

One of the possible options for the fissile fuel resource is the fusion-fission hybrid (slide 5). This has been an important area of work for us at EPRI. Recently we have conducted an extensive evaluation of this hybrid issue. Last year we had a high level review panel that told us that if we wanted to get into the hybrid issue, get into the fuel factory. They told us not to worry about hybrids for producing baseload power. Unfortunately after that review there were unresolved issues with respect to our evaluation of fusion-fission hybrids. I have listed five of them here. Our management at EPRI doesn't see that there is any need for hybrids. They see for the foreseeable future plenty of fissile fuel. They see in the present atmosphere of light-water reactors that projections for the midterm future will not create serious demands. However, I am only speaking about the EPRI management. I am not speaking for the utilities.

There is at least one alternative to fusion-fission hybrids, spallation breeding. Some of EPRI management feels spallation breeding is a more direct way to obtain fissile fuels than fusion-fission hybrids. We have not evaluated that issue completely yet.

Third, there have been in the last 10 years new discoveries of uranium both in Canada and Australia. The geological types of discoveries make it uncertain how much, is really out there. At least the uncertainties in the amount of uranium are on the top side rather than the bottom side. However it still is an uncertainty.

Fourth, it seems unfortunate for fusion that the light-water reactor community hasn't completed its fuel cycle. We worry very much about the reprocessing that has to be done for any hybrid, the waste disposal and the type of hybrid fuels that would be produced. Can the hybrid-produced fuel be handled with the processing in existence? Would the fuel really be the kind of fuel LWR's want to use? Although we have recently conducted workshops on these issues we have not come to conclusions.

Finally, as I mentioned before, there are two companies doing a comparison of the development of fusion-fission hybrids as compared to fusion electric. We want to quantify that statement to see if indeed it might be appropriate to go the direction of fusion-fission hybrids.

EVALUATION OF MCF ENGINEERING OPPORTUNITIES

- Selected 24 reactor designs for study
- Identified 78 Critical Engineering Data Needs (by Designers)
- Identified 28 experimental areas available for obtaining data
- Defined 80 experiments to obtain critical data
- Prioritized and selected 10 experiments for EPRI consideration

SLIDE 6

With the change in our program in the last several years we are exploring engineering activities (slide 6). We recently completed a project which we think is very interesting. We have evaluated what we call engineering opportunities in the fusion program. This viewgraph lists the tasks of this particular project in sequence. We hired McDonnell Douglas to take a look at what possible engineer-

ing activities EPRI could do in the fusion program. We also did the same thing with Westinghouse on inertial fusion.

The first task was to talk to the reactor designers to find out where their data was weak and what would improve the quality of their designs by having better technical data. We identified 78 engineering data needs. I am sure there are even more than that. But tne designers picked what they considered very serious areas that they would like to improve.

We then went to the people at the facilities, people in laboratories, and also in the nuclear industry and other industries to see where these data could be obtained by experiments. We defined 80 experiments. Then we prioritized the list for EPRI activity. There are many of these 80 experiments in the national program, or will be in the national program. It was not reasonable nor sensible in terms of dollars for EPRI to be involved. We identified 10 experiments for EPRI.

A good example is tritium permeation. It isn't a serious problem for facilities that now exist or will be existing in the national or international program. But if you go to high temperature first walls, tritium permeation becomes a problem. This is what I would call a downstream problem that EPRI and the involved utility personnel feel they would like to know more about it. So we are actually initiating a project at EPRI on this issue. There is a problem of hydrogen diffusivity through solid tritium breeding materials that we also are examining. Obviously we can't do all of those.

Another major project EPRI is doing is a project called technical risk assessment. This is a project that, on the surface, seems to be what you might call challenging to the other programs. It is not intended that way. The utilities and EPRI feel strongly about being sure the fusion fuel cycle is completed. They don't want to be faced with the same problem with fusion they are presently faced with with fission.

So we were encouraged to look at the fusion cycle from the viewpoint of what is the commercial system fusion fuel cycle going to have to be, what is its specifications, how many kilograms of tritium do you have to have and how are you going to handle it, and what kind of components do you have to develop.

We first specify the commercial system. Next, we look around to see what candidates, subsystems, and options are available. We are doing this now. The third step is to conduct technical forecasting of the state-of-the-art of each of these subsystems to find out where they are now and what the technical people feel has to be done in order to reach the specifications of the fuel cycle as a commercial system.

We would evaluate these, selecting the most optimistic, and then identify critical R&D needs for these and develop an R&D plan to ameliorate these risks. EPRI then would select some area for further activity that would be of use of EPRI and the utilities.

My final project, which takes a third of our budget is the blanket module on TFTR. It has a budget of approximately $1 million a year. This is a cooperative effort between EPRI and the Department of Energy and Princeton. It is indeed important from our viewpoint because it is one of the first engineering experiments that will establish the data base and the computational base for tri-

tium breeding. It has a budget which totals $4.75 million in 5 years. We are very pleased to be able to participate in this way on TFTR.

There are two more important points I want to make. We are doing at least two activities in inertial confinement. As I said before, we try to keep a balance between magnetic and inertial. The work we do in inertial is directly aimed toward producing power and doesn't have anything to do with the national program and its particular near-term objectives. But I will leave you with the idea that we have to keep our eyes on inertial confinement and understand how it is developing, because whether we like it or not, we know it works. It is really a matter of scale, to get the size down, and to get the associated technologies available to see if indeed it can be handled on a small scale.

In terms of conclusions, EPRI does see the national and international fusion program as a mature program. What I mean by that is the research facilities that they build now work the way they had in mind to have them work. This is a mature program. It is in the early stages of developing its engineering capabilities. We certainly want to be involved from the utility viewpoint in some of the decisions in the near and far term.

Thank you.

[The prepared statement of Dr. Scott follows:]

Prepared Statement of F. R. Scott

The primary objective of EPRI's Fusion Power Systems Program is to ensure the identification, selection, and development of fusion systems that can be used by the electric utility industry. Recent organizational changes at EPRI, as well as an Energy Resource Advisory Board review of the federal fusion program, have prompted a reappraisal of the role and strategy of EPRI's fusion research. This reappraisal has involved the utility advisory committees, an independent review group, and Institute staff at all levels. The resultant program will focus on defining engineering issues crucial to the development of practical, economical, environmentally acceptable fusion power systems that have the reliability, availability, maintainability, and electrical characteristics essential for integration into utility systems. EPRI will closely coordinate its program with DOE, which is beginning the engineering activities necessary to develop fusion systems that can be used in practical applications.

In the context of the large, rapidly developing international research effort, the EPRI Fusion Power Systems Program is small with an annual budget of $2 million in 1982. Since 1974 the EPRI program has sponsored 41 projects which have included investigations of first wall blanket systems, remote handling, preliminary small systems design, low activation materials, hybrid applications, alternate concepts and advanced fuel systems.

The present program is emphasizing five major objectives:

- o Identifying and evaluating technical issues crucial to the engineering development of practical fusion power systems for the utility industry
- o Encouraging innovative, utility-oriented approaches to the engineering development of fusion power
- o Performing state-of-the-art assessments and communicating them to the utilities
- o Maintaining close links with DOE and defining appropriate engineering experiments to complement the federal program
- o Supporting engineering project activities in areas common to all fusion technologies.

The program highlights utility-oriented issues, such as power plant size, compatibility with utility grid, and maintainability. The major thrust of DOE's program has been large (1-GW (e)) fusion power plants. The development of concepts in support of small systems could improve fusion's ultimate penetration capability as well as improve the ability of the program to obtain engineering experience on modest facilities.

To implement its goals, the EPRI program is organized into three subprograms: magnetic confinement fusion, inertial confinement fusion, and analysis and technical development.

The magnetic confinement fusion subprogram complements the much larger federal program in this area, which is pursuing the goal of scientific feasibility. The EPRI subprogram is performing technical risk assessments of selected engineering activities, identifying and supporting the development of promising magnetic concepts, and providing performance evaluations of magnetic fusion experiments. For example, several small-reactor concepts have been evaluated. At present a technical risk assessment of the fusion fuel cycle is in progress. This will lead to identification of critical tasks for R&D.

Similarly, the inertial confinement fusion subprogram complements a much larger federal program. Present federal efforts are aimed first at establishing the scientific credibility of inertial confinement and applying it to weapons research--a goal that requires the construction of large driver systems, an examination of basic physics of driver-target interactions, and the design of realistic targets. Ultimately the federal program expects to examine the energy applications of inertial fusion.

The major goal of EPRI's subprogram is to define technical and preliminary engineering activities that will help determine the potential of inertial confinement fusion for power production. Specific projects involve the identification of utility-preferred systems, assessments of the developing technologies expected to have utility applications, and performance evaluations of existing and planned systems and components. Under one completed project, for example, inertial fusion drivers (lasers, light ions and heavy ions) were assessed. One conclusion was that the light ion approach appeared to offer a most desirable, small system (and hence lower cost) commercial developmental path. Consequently, a current project is defining the desirable and achievable characteristics of a 1990's light ion engineering test reactor system. Other projects are focusing on the identification of critical engineering data needs and possible experiments to obtain it. Finally, in an attempt to define the R&D required to bring inertial fusion from its present scientific and military emphasis to one of potential commercial application, a technical risk assessment is underway on the three driver approaches.

The analysis and technical development subprogram emphasizes activities relevant to both magnetic confinement and inertial confinement fusion. Like the other two subprograms, it evaluates new technical approaches, conducts technical risk assessments in selected areas (one important example is fusion-fission hybrids), and develops state-of-the-art information for utilities. It is also planning to conduct engineering experiments at existing fusion facilities--for example, blanket engineering tests on the TFTR. By comparing alternative engineering development paths, this subprogram seeks to determine the most effective route to useful fusion power.

In cooperation with DOE and the Princeton Plasma Physics Laboratory, EPRI has initiated a project to define fusion blanket engineering tests using the TFTR's toroidal fusion neutron source. The general objectives of this effort are to conduct a series of neutron transport and tritium-breeding experiments with a lithium blanket module (LBM) and to compare data obtained with predictions of reactor blanket design codes. The LBM research will also determine how the design codes and neutronics data should be improved in order to bring calculations of performance into closer agreement with experimental results.

The program of experimentation and analysis, the first of its kind using a controlled fusion device, has the potential to significantly increase confidence in fusion reactor blanket design concepts and to demonstrate the feasibility of closing the D-T fuel cycle. It is also expected to provide reactor-prototypical experience in several key areas, including fabrication of tritium-breeding elements, blanket instrumentation, remote handling of blanket modules, electro-magnetic isolation of blanket modules from the tokamak plasma, and safe integration of blanket module into a D-T tokamak reactor assembly.

A major part of the fusion program is expected to involve technical risk assessments of near term engineering systems. The following general tasks for such a project are involved.

- o Define the objective of the system
- o Identify appropriate systems and their components
- o Complete specific designs of selected systems
- o Conduct operational and systems analysis for each system
- o Identify key scientific, technical and engineering risks (uncertainties)
- o Develop the data base for relevant scientific, technical and engineering issues and conduct technical forecasting for critical needs
- o Develop and apply a methodology for optimizing the system or systems to meet the objective
- o Compile a detailed research plan for reaching the objective

Although fusion subsystems are not yet sufficiently defined to permit detailed failure risk analyses, the general engineering approach represented by these tasks can be used to identify technological deficiencies that must be eliminated. A completed analysis should provide plans for correcting any deficiencies found.

In conclusion the present EPRI Fusion Power Systems Program is focusing on engineering issues critical to the development of practical, economical and environmentally acceptable power systems. We expect to be an independent, user-oriented participant in the early development of fusion power.

Mr. RICHARDSON. Thank you very much, Bob, for that overview. I think the participation of the utilities is most important and cannot occur too early, as you indicated.

Are there comments or questions for Bob?

Dr. WOODSON. Bob, you ought to fix your map because it shows 8 dots in Texas but there are 10 investor-owned electric utilities in Texas that support fusion.

Dr. SCOTT. I think the explanation of that was that they didn't count Texas utilities, they only counted them as one utility.

Dr. WOODSON. Yes, but they are there.

Mr. GOODRICH. Herb, if you want to add dots that way, you can add a whole lot more because there are a lot of holding companies.

Mr. REICHLE. Is there any prospect the EPRI fusion budget will be increased?

Dr. SCOTT. We are always hopeful of that. Yes, I think so. I don't know how much it will be increased but we are certainly very optimistic. We are always working on it. It is a matter of the priorities at the institute. The institute has certainly focused in the last few years on near-term issues, near-term activities. I think that has been partly due to the institute and partly due to the utilities. There have been a lot of problems with the utilities in the last few years. EPRI has been doing its best to help them. We hope that those problems disappear and they become more interested in fusion.

Mr. RICHARDSON. Mr. McKenzie?

Mr. MCKENZIE. EPRI seems less interested in fission-fusion than DOE seems to be, if I read this right.

Dr. SCOTT. I would say just the opposite. In terms of total dollars EPRI's fusion program has spent something like—$6 million over the last 5 years.

Mr. MCKENZIE. I am sorry, maybe I didn't hear you right.

Dr. SCOTT. The EPRI fusion program has emphasized the hybrid, yes, much more so than the Federal Government.

Mr. MCKENZIE. I thought you said there seemed to be so much uranium——

Dr. SCOTT. At that moment I was only listing the uncertainties we see in our evaluation of the hybrid. We are working on it because the utilities tell us that they want us to do these things. On the other hand, how much of a major commitment should we make if there is no need? The perception of EPRI management is that there is really no need. I think John Foster some years ago put it

plainly. You have got to have a customer. We need a good, viable, light-water reactor industry to be sure we have got a customer.

Mr. RICHARDSON. One more comment from John. We are racing the clock in order to get to lunch.

John?

Dr. CLARKE. I was just curious. Did you say that there is more interest in EPRI management over spallation breeders, than fusion hybrids? I thought I heard you say there is no interest in the need for breeders. I don't understand the contradiction.

Dr. SCOTT. I was referring to some of our managers and their perception that if they had to make a choice in producing fuel, they would look at spallation in some cases as more near-term technology than hybrids.

Mr. RICHARDSON. We will now proceed to the other soloist in this part of the program. As I understand it, the utilities are the ultimate users. But there is a myriad of additional industrial sectors out there that have to serve as an infrastructure. Perhaps Frank Graham intends to address this point.

Frank is currently the special studies manager for the Atomic Industrial Forum. He was previously the director of nuclear planning for Con-Ed in New York, and before that, has apparently had a full career with the U.S. Navy, commanding several of the Navy's submarines.

Frank, would you like to give us your remarks now, with a view toward the clock? I would say that some 15 minutes or so for presentation followed by 5 or 10 minutes for discussion. Then we have to leave for lunch about 12:40.

STATEMENT OF FRANK GRAHAM, ATOMIC INDUSTRIAL FORUM

Mr. GRAHAM. John, I will try to cut it fairly short. I was asked in my role as secretary of the Committee on Fusion, the Atomic Industrial Forum's Committee on Fusion, to say a few words on the committee's work and on its perception of the fusion program. The chairman of the Committee on Fusion is Sid Law, Director of Research for Northeast Utilities.

You have perhaps noted that there are also in attendance other members of our committee. They represent the basic industries that would be necessary to commercialize fusion power. Utilities that would own and operate the plants, architect/engineers who would design and construct them, and the manufacturers that would manufacture the hardware. The committee members are experienced in large energy projects, and they are knowledgeable in the fusion program.

The committee was established near the end of 1974, about the same time as the last major forward steps in the fusion program were started—the Doublet III and the Tokamak Fusion Test Reactor. Bob Hirsch, who was then director of the Office of Magnetic Fusion in the Energy Research and Development Administration, asked AIF President Carl Walske if the forum would establish a committee to provide an industry perspective of the growing fusion program. The forum asked Ted Brown to review the program's status. He did, and recommended that an AIF committee be established. The AIF board of directors approved the committee in De-

cember 1974. The committee members visited the national laboratories, Princeton, General Atomics, KMS Fusion, universities—some a number of times. They have prepared four reports which indicated the progress of the fusion program and industry's role. All four of the program reports have in common two major threads.

The first is recognition that the ultimate goal of fusion is its commercial application to meet the energy needs of the country. Second, that engineering technology, which is focused on commercial application, be developed in parallel with the science.

The first report urged an increased industry involvement to utilize industry's existing capabilities in conventional areas and simultaneously permit industry to develop new capabilities consistent with the emerging technologies, a very important role for industry. It noted that the scientific and engineering development programs were in balance at this particular point in the development of fusion.

Our second report was completed in April 1978. It was in reality an update of the first report. It recognized the very highly productive nature of the fusion program during that period. Experiments and theoretical results were coinciding, scaling laws were coming into focus. There was high confidence prevailing that science would achieve an energy output. At this time the committee also expressed the concern that the necessary base of technology and engineering would not keep pace.

The third report was completed in December 1979 and was perhaps the most significant, at least in my view, of the reports that the forum has made. It recognized that the advances in science at that time strongly supported the conclusion that fusion energy could make a major contribution to meeting the Nation's energy needs, and that there was confidence that fusion energy could produce net power before the end of the century. The report noted that a major engineering and technology effort was needed, and that an engineering test facility which was then being planned by the Department of Energy, would mark the transition of fusion programs from a focus on scientific research to engineering and development of hardware.

The report also reached the conclusion that it was a logical and overall cost-effective time to start the follow-on engineering test facility. The conclusions of this report were supported about a year later by the Buchsbaum panel report. That report, as you well know, was the basis for the Magnetic Fusion Engineering Act of 1980.

The fourth report, which was completed in January 1981, followed passage of the legislation and the committee at that time strongly supported the establishment of a Center for Fusion Engineering. The report emphasized again the necessary focus on the commercial use of fusion power.

Let me close by summarizing the committee's perspective as viewed today.

Fusion has considerable credibility in the industry. The committee believes that the program is at its most cost-effective point to move forward. However, the committee had determined that the program was at that most cost-effective point 2 years ago. Unfortu-

nately, the program has not gone forward, and we have lost 2 years. We believe the scientific evidence supports proceeding now with parallel engineering technology development, that engineering technology development be oriented from the outset toward commercial application, and that the Center for Fusion Engineering is a critical step in achieving that.

Last, the committee has noted that funding should be program paced for the most effective development of fusion.

Mr. RICHARDSON. Well, you did beautifully.

Mr. GRAHAM. I could add more if you wish to take the time.

Mr. RICHARDSON. We can hang around. We are not that pressed for time, Frank. Do you have any more points you would like to make?

Mr. GRAHAM. No; I think it would take more time than we have.

Mr. RICHARDSON. Would you like to entertain discussion at this point on the participation and role of industry, which I know are of great interest to the workshop organizers. Questions for Frank on this point?

Dr. SMITH. I want to go back to the Center for Fusion Engineering. AIF was really the originator of that in my remembrance. Yet the justification was primarily for computing facilities, office facilities—are there other justification for that kind of a center?

Mr. GRAHAM. I don't think the AIF Committee on Fusion was the originator for the Center on Fusion Engineering. I believe I am correct in saying it was the Buchsbaum committee that developed the concept of the center.

Dr. SMITH. I first heard about it from Ted Brown and nuclear people meeting with Senator Church before the Buchsbaum report was released.

Mr. GRAHAM. As I remember, we talked about a management structure that would be necessary for parallel development. I would like to take credit for it.

Dr. SMITH. Well, let me ask DOE what the Buchsbaum report proposed the Center should do besides saving duplication.

Mr. GRAHAM. May I interrupt just a minute, John? Following the Buchsbaum committee report, we did a study on the role the CFE should have. That role was to coordinate all of the engineering; that is, there are two points that I think are key. One is that engineering technology should be focused toward the commercialization of fusion power. In focusing on commercialization there needed to be some institutional arrangement that could gather together all of the engineering data being developed, and see what else was needed. Eventually the CFE centerpiece would be an engineering test facility where a great deal of the experimental work for and development work for engineering technology could take place.

Dr. SMITH. Thank you.

Dr. CLARKE. I am trying to remember exactly what the Buchsbaum committee said about the Center for Fusion Engineering. My recollection is that the concern was that they were recommending moving the program into what they characterized as the new phase of engineering technology development. I think they felt the new organizational structure should be put together to manage that particular development.

Dr. SMITH. I think history will show that Ted Brown was pushing the idea before Buchsbaum got to it. We put it in the authorization bill at least 1 year before the engineering bill was passed requiring a study of the concept. I have a vivid memory that we met in our committee library with some people from New Jersey, two or three other people, and Ted Brown, who were advocating it. Then Mr. Shapiro from Westinghouse asked if we would stick our neck out and push with this.

Dr. DUGAN. In the absence of that kind of institution what is the industry view of the role that the Fusion Engineering Design Center is taking in the program?

Mr. GRAHAM. That is a very important aspect obviously because there are a number of industries contributing to that Design Center, and I think it is a very valuable contribution that they are making. That of course has been looking specifically at the design of a device and updating it as new information becomes available. Very important.

But the Center for Fusion Engineering would have had a much broader mandate. It would look at all of the engineering technology that is going to be needed for a commercial plant, and direct engineering development including operational requirements, maintainability, the cycling of all of those factors toward that end. The Center for Fusion Engineering would be the focal point to insure all of those things were done on schedule, and that they match the progress of science and scientific development as we learn more about plasma physics.

The CFE is a different concept from the Design Center, although certainly, the function of the Design Center is included in a Center for Fusion Engineering.

Dr. DUGAN. But is there any reason, other than funding limitations that the Design Center couldn't widen its scope of activity?

Mr. GRAHAM. It certainly could, Jack.

Dr. DUGAN. John may want to comment.

Mr. GRAHAM. It could do that. The Center could develop from that design effort. It would really depend a great deal upon how the program is structured over this interim flat budget period. What is the Design Center going to do, what its function is going to be, what kind of funding is necessary for it, and what kind of organizations and institutions are going to develop to manage the engineering technology through the 1980's?

Mr. RICHARDSON. Joe?

Mr. GAVIN. At the risk of being controversial, with respect to the Center for Fusion Engineering, it seems to me fundamental that the program should demonstrate the need for such an institution, rather than to have the feeling that the program depends on the existence of such an institution. I think this should be looked at in the context of the fact that, if I have the figures correct, there are something on the order of 700 federally supported laboratories around the country. It would seem to me that, before something new is established, a good look should be taken at what exists to see what can be adapted. I think this is just another part of making the best of what is currently available as we go along.

I think the same economic drive is going to put a great premium on the upgrading of current experimental facilities and the making

of maximum use of existing centers. That is perhaps not as bold a view as some people would like to hear, but I think it is a very practical one.

Mr. RICHARDSON. Steve?

Dr. DEAN. Yes; if this is an appropriate time I would like to just make a 1-minute statement to complete the record on the nature of Fusion Power Associates and how it may differ slightly from the AIF, since this is a question that is frequently asked.

I think we differ in two respects, although many of our members and many of our activities are the same, or at least complementary. The first is that the AIF is incorporated and recognized by the IRS as a 501(c)(6) organization, which means that it is a trade association. That means that the IRS looks with favor on its activities to the extent that they are allowed to be tax exempt while at the same time doing things which benefit their industry as a trade.

Fusion Power Associates is not a trade association. We are what is called a 501(c)(3) organization, which means that we are a scientific and educational organization whos primary function is to benefit the public. So that when the IRS looks at our activities and our policies, we must be able to demonstrate that what we do has the primary purpose of benefiting the public, rather than benefiting our members or our industry.

The second way in which we differ, and here I think we also differ from EPRI and DOE, is that our sole function and interest is in fusion; whereas the AIF's primary function, simply by its history, is fission, and fusion is only a part of their interest; and EPRI's primary mission is a variety of things of interest to the utilities, and fusion is a only part of that; and DOE's primary mission is energy, fusion is only a part of that; whereas our sole purpose and objective is to expedite fusion.

We incorporated in 1979 with 10 members. We now have 51 corporate members and affiliates, and over 200 individual affiliates. I have a list of our board of directors, our purposes and organization which I would like to simply submit for the record.

[The information follows:]

FUSION POWER ASSOCIATES

2 PROFESSIONAL DRIVE, SUITE 248 • GAITHERSBURG, MARYLAND 20879

(301) 258-0545

March 1982

Fusion Power Associates is a non-profit, tax-exempt corporation established to ensure the timely development and acceptance of fusion as a socially, environmentally and economically attractive source of energy. To fulfill this purpose, we have adopted three primary goals:

- **To ensure a smooth, timely transition from scientific research on fusion to engineering development and practical application.**
- **To foster cooperation in fusion research and development among all public and private organizations including government, universities, national laboratories and industry.**
- **To establish increased public awareness and understanding of the potential of fusion energy.**

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Mr. RICHARDSON. It seems clear to me that there is sort of an escalation ladder of industry involvement here. At the low end of the ladder you can think simply of scientific exchange in which the mode would be some sort of synergism that exchange scientists would bring from one program to another. On next rung of the ladder, there would be individual contractors, making components and providing services at the direction of the Department. Then industry as program manager has been mentioned. That seems to await full-scale development, or a decision to go to fullscale development.

Another thing that has been mentioned is the forging of the necessary and massive infrastructure to support a mature fusion industry. This might be analogous to the massive infrastructure that grew up in support of nuclear propulsion. Finally, there would be industry as an investor in its own right. So far, on this ladder of escalation, I have not heard much discussion about appropriate magnitudes for the present or as a function of time in the future. Should there be 2,000 people involved in industry today?

Ken?

Dr. FOWLER. Not so much a view as a number for the record. John, didn't we obtain from Japan some estimate of industry involvement? I don't remember the numbers but it was an astounding number. It would be worth bringing out if we could remember what it was.

Dr. CLARKE. I don't recall.

Dr. FOWLER. It must approach at least one on one relative to their scientific university involvement, if my recollection is correct.

Dr. CLARKE. That is probably right. But of course, remember that Japan has been in the construction stage for the last 5 years and they do their construction through industry. They don't have in-house engineering construction capability in the laboratory. So that would explain the large ratio. But as I indicated before, that if you look at where the dollars go, half of our budget ends up in the hands of industry. So industry is involved. Half the effort in the United States is industrial. I think the difference is in where industry comes in and where the responsibility lies.

Mr. RICHARDSON. Well, I think that has been a good morning's work. Our plan now is to break for lunch until 1:45. That is a 15-minute slippage from the originally announced schedule. We expect that our visitors from Japan and Europe will be with us at 1:45. There has been a lunch prepared for the participants and the panel in room 2325, this building. I believe I am correct in saying it is catch as catch can for the observers. Sorry about that.

See you at 1:45.

AFTERNOON SESSION

Mr. RICHARDSON. Ladies and gentlemen, could we reassemble now to begin the afternoon session? Would you take your places for the afternoon session?

After an interesting morning and a most gracious and delicious lunch provided by the two subcommittees, we want now to move to an exceedingly important part of the fusion effort; namely, the international cooperative aspects.

We are fortunate that the timing of this meeting coincided with a meeting of the International Atomic Energy Agency being held in Baltimore and this gave us the opportunity of having two distinguished guests from overseas. These guests are Dr. Sigeru Mori and Dr. Donato Palumbo from Japan and Italy, respectively.

The first speaker will be Dr. Sigeru Mori, who is the head of the Division of Thermonuclear Research, the Japan Atomic Energy Research Institute, and his general topic is the international perspective on magnetic fusion energy.

Dr. Mori?

STATEMENT OF DR. SIGERU MORI, HEAD, DIVISION OF THERMONUCLEAR RESEARCH, JAPAN ATOMIC ENERGY RESEARCH INSTITUTE

Dr. MORI. The first viewgraph is on the wall (slide 1). First I would start from the fusion objective for Japan and this is for solving the energy problems of Japan because the Japanese country is much less endowed with fossil and fissile resources than the rest of the world. The area of the country is very limited, of course, so we are very concerned about that and about environmental preservation and safety.

FUSION FOR JAPAN

* For solving Energy Problems

Less endowed with fossil and fissile resources.
An environmental preservation and safety.

* For establishing high-technology-based country

A concentrated investment for the frontier-technology R&D.
A driving force for and a suitable target of high-technology.

Our nation has been promoting nuclear fusion research and development as the National Project, aiming to achieve the two major objectives mentioned above, early in the 21st century. We believe that the international cooperation is quite important for the rapid and efficient progress of the nuclear fusion research and development.

SLIDE 1

The next reason for pursuing fusion is for establishing a high technology based national economy. It is true that we have a large population and our resources are very scarce, so our policy is to establish the high technology base for our country. In this context, our concentrated investment for frontier technology and technology R&D is going on in Japan. Also, fusion is the driving force and a suitable target for high technology, as you are aware.

Our nation has been promoting fusion research and development as a national project aiming to achieve the two major objectives mentioned above. We are striving to achieve this early in the 21st

century. We believe that international cooperation is quite important for the rapid and efficient progress of the nuclear fusion research and development.

The next slide contains a short history of the Japanese fusion program (slide 2).

1955 -	Institute of Plasma Physics, Nagoya Univ. (from 1961) (1)	-formation of fusion community (2) -systematic research on plasma physics -training of young researchers (3)
1968 - 1974	Special Integrated Research in Atomic Energy (AEC) (4)	-mission oriented R&D
1975 -	National Research in Atomic Energy Research and Development (AEC)	-project

(1) A center for cooperative research among universities.

(2) Disciplines of the members were accelerator physics, physics and engineering of electrical discharges, electrical engineering and theoretical physics.

(3) Special grant and fellowship for fusion were volunteered by two newspaper companies.

(4) A first action of Atomic Energy Commission to promote fusion research.
The Research was composed of toroidal confinement of low- and medium-beta plasma by Japan Atomic Energy Research Institute (JAERI), plasma diagnostics by Institute of Physical and Chemical Research, and toroidal pinch by Electro-Technical Laboratory.

SLIDE 2

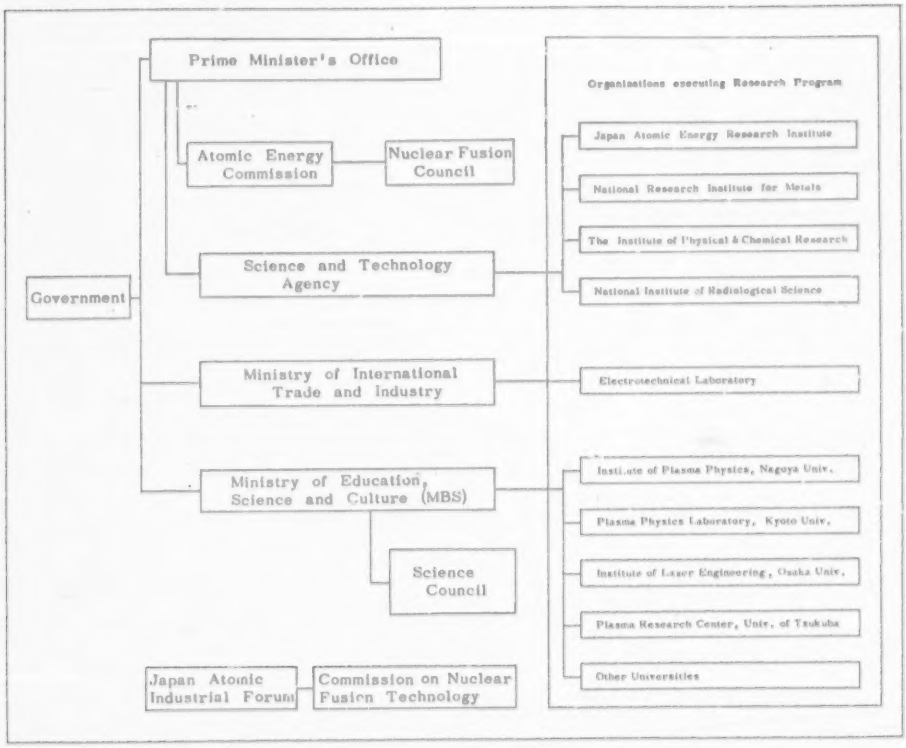
We started research in 1955 and we etablished the Institute of Plasma Physics at Nagoya University in 1961. In this way we formed a fusion community. Most of the early members were astrophysicists who trained young researchers on plasma physics.

Afterward, the Japanese Atomic Energy Commission picked up this responsibility with emphasis on fusion, which was a second priority in these activities.

From this period, fusion research in Japan stepped into a mission-oriented phase. It was mainly for basic research prior to that. We made good progress in this special area and progressed into the project phase in 1975.

This is a top priority project in Japan and it is continuing vigorously.

Next slide, please (slide 3).

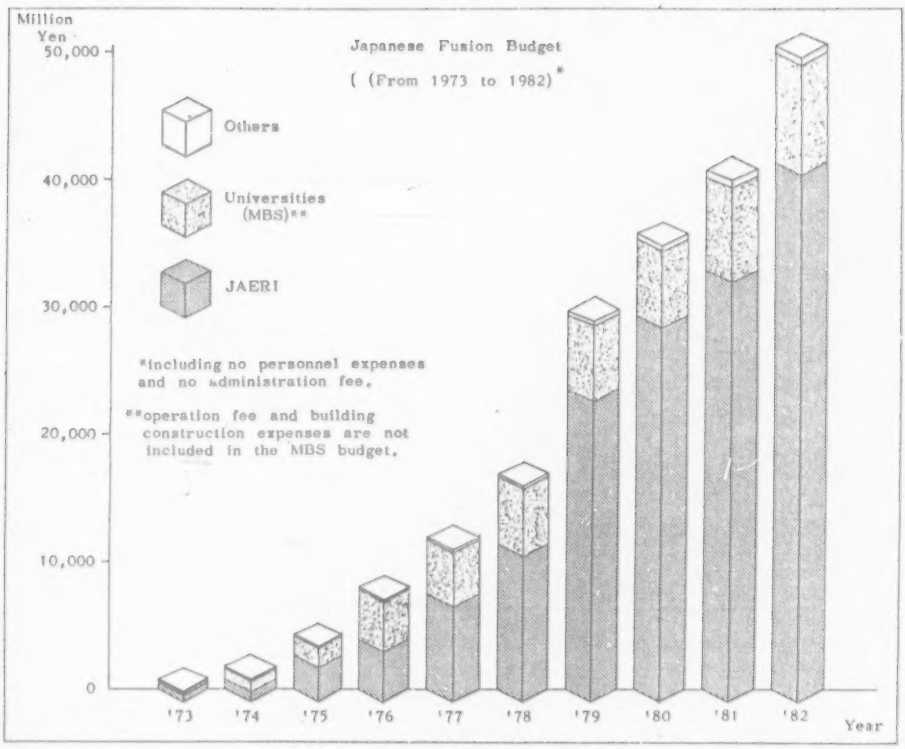

Prime Minister's Office
Atomic Energy Commission
Nuclear Fusion Council
Science and Technology Agency
Government
Ministry of International Trade and Industry
Ministry of Education, Science and Culture (MBS)
Science Council
Japan Atomic Industrial Forum
Commission on Nuclear Fusion Technology
Organizations executing Research Program
Japan Atomic Energy Research Institute
National Research Institute for Metals
The Institute of Physical & Chemical Research
National Institute of Radiological Science
Electrotechnical Laboratory
Institute of Plasma Physics, Nagoya Univ.
Plasma Physics Laboratory, Kyoto Univ.
Institute of Laser Engineering, Osaka Univ.
Plasma Research Center, Univ. of Tsukuba
Other Universities

SLIDE 3

I am sorry this is difficult to see, but this shows the organization of the fusion research in Japan. Mainly, you can see the Atomic Energy Commission in the second level and Science and Technology Agency (STA) down below that. The STA is one of the major ministries for fusion.

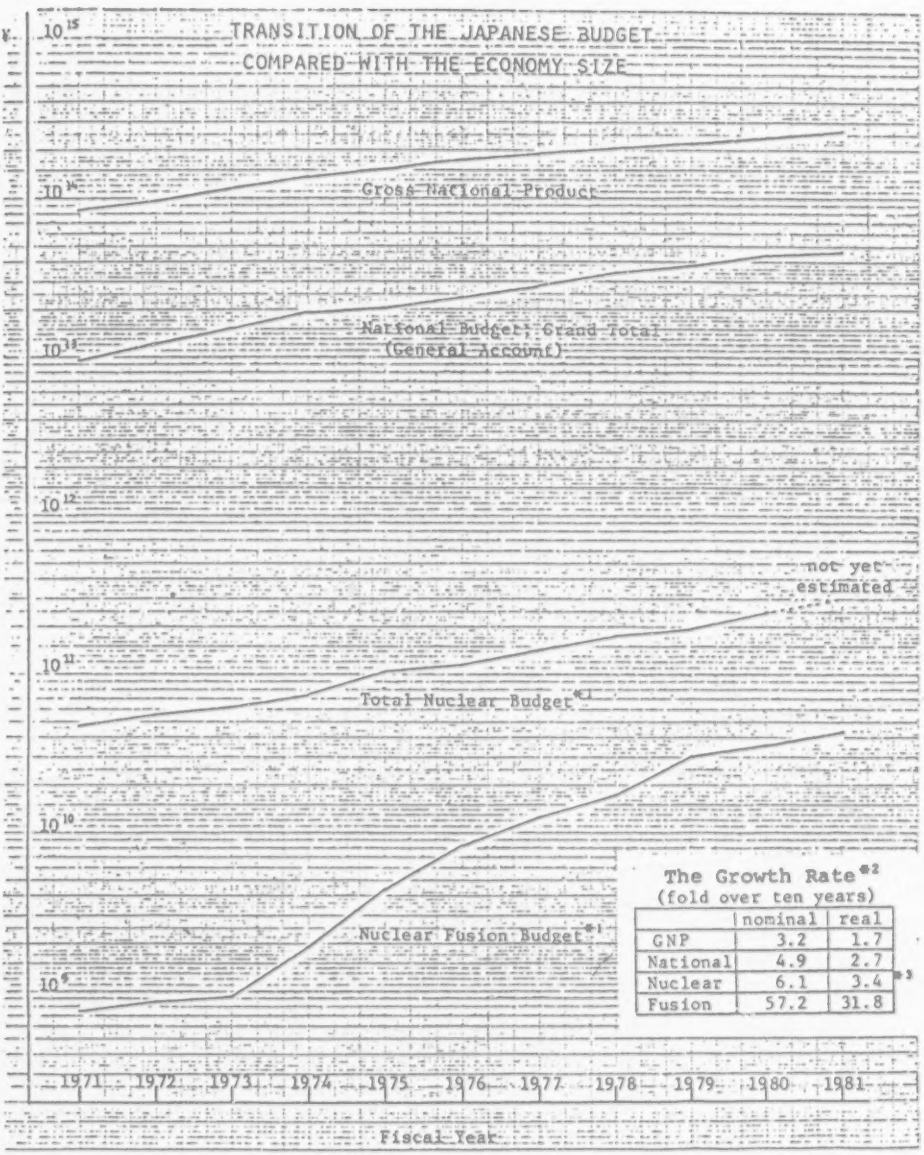
The next level is the Ministry of Education, Science and Culture. The Ministry of Education projects are mainly ones that come through the universities.

Next slide, please (slide 4).



SLIDE 4

This shows the evolution of the Japanese fusion budget from 1973 to 1982. At the top is 50 billion yen, which corresponds to about $200 million, if the exchange rate is 250 yen to $1. The reason for rapid increases after 1979 is that the Japanese activity evolved through the project phase. I must say that this budget includes no personnel expenses and no administrative fees; and also operational fees and building construction expenses are not included in the budget for Monbusho, which is the Japanese name for the Ministry of Education and Culture.



The Growth Rate*2
(fold over ten years)

	nominal	real
GNP	3.2	1.7
National	4.9	2.7
Nuclear	6.1	3.4 *3
Fusion	57.2	31.8

*1 Including R&D on National Institutes (STA), National Universities (MOE) and government subsidy for nuclear power plants (MITI), but excludes industry's (commercial) investment.

*2 1971~1981, inflation ratio: 1.8 fold (rough estimation)

*3 1971~1980.

SLIDE 5

This slide is hard to see (slide 5), but this is the transition of the Japanese budget compared with the size of the economy. The top line represents the gross national product, and the second line shows national budget totals, the grand total. This is only for the

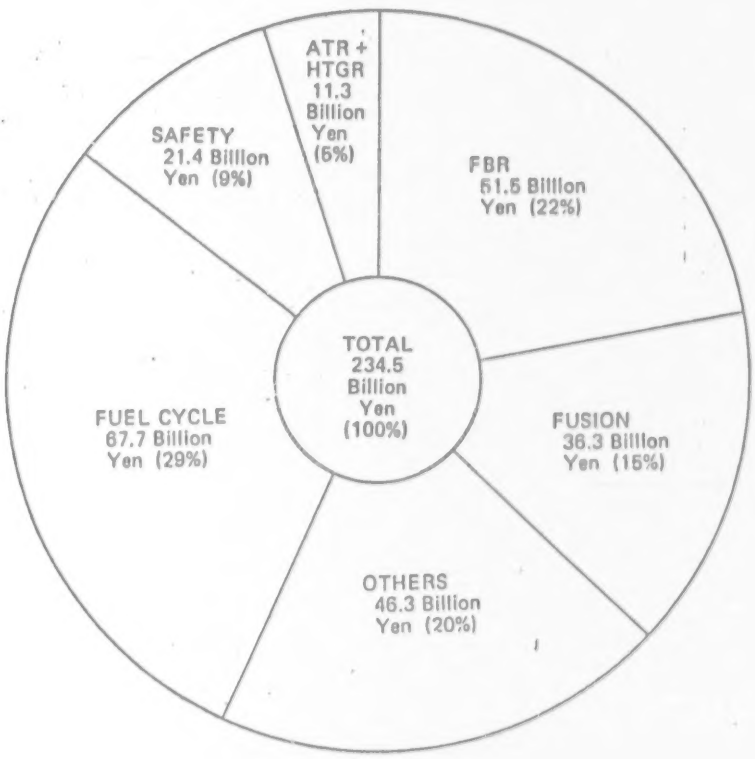
general account. As you know, the Japanese Government has another accounting system with a special account.

In the fourth line, you see the total nuclear budget.

The bottom line is the nuclear fusion budget.

So you can see the share of the nuclear fusion is increasing very rapidly.

BUDGET OF SCIENCE AND TECHNOLOGY AGENCY
FOR FUSION AND FISSION R & D, FY1981


ATR + HTGR 11.3 Billion Yen (6%)
FBR 51.5 Billion Yen (22%)
SAFETY 21.4 Billion Yen (9%)
TOTAL 234.5 Billion Yen (100%)
FUEL CYCLE 67.7 Billion Yen (29%)
FUSION 36.3 Billion Yen (15%)
OTHERS 46.3 Billion Yen (20%)

SLIDE 6

This slide is perhaps more understandable now (slide 6). It shows the budget share in science and technology for fusion and fission R&D projects. The fission breeder reactor (FBR) has 22 percent, fusion has 15 percent, and also we have safety activity with 9 percent and fuel cycle work at 29 percent.

There is a tendency that some part of the FBR activity and also some part of the fuel cycle activity is going to the private firms to be commercialized. The necessary expenses for the FBR and fuel cycle are borne by the private companies. For example, 20 percent of the cost of the construction of the prototype FBR reactor comes from private funds.

So if we take the long-term view of this, I think the fusion and other fission concepts such as the, HTGR, or high temperature, gas cooled reactor, will have a larger share.

MAJOR FUSION FACILITIES AND THEIR MANUFACTURER

Type	Device	Institute*	Manufacturer	Cost(B¥)**
Tokamak	JFT-2	JAERI	Hitachi	—
	JFT-2M	JAERI	Mitsubishi	3.9
	JFT-2a	JAERI	Hitachi	—
	JT-60	JAERI	Hitachi,Toshiba, Mitsubishi et al	180
	Doublet-III	GA		≥17.5***
Tokamak-Stellarator	JIPP-TII	IPP	Hitachi	—
Stellarator	Heliotron-D	Kyoto	Hitachi	—
	Heliotron-E	Kyoto	Hitachi	9.6
Tandem Mirror	Gamma-6	Tsukuba	Toshiba	—
	Gamma-10	Tsukuba	Toshiba	6.2
Laser Fusion		Osaka		12.7****

* JAERI: Japan Atomic Energy Research Institute
IPP: Institute of Plasma Physics, Nagoya Univ.
Kyoto: Plasma Physics Laboratory, Kyoto Univ.
Tsukuba: Plasma Research Center, Univ. of Tsukuba
Osaka: Institute of Laser Engineering, Osaka Univ.

** 1 B¥ = 4 M$ (250¥ = 1$)

*** Japanese share of the expenses for participation in Doublet-III Collaboration between DOE and JAERI during five years project

**** Research budget accumulated from 1973 to 1982

SLIDE 7

The next slide (slide 7) shows the participation of private firms in the JFT-2 and JFT-2M which are the major facilities in Japan and are manufactured by Hitachi, Mitsubishi, and Toshiba; these companies are, I suppose, well-known in the United States and are five of the biggest firms in Japan.

I put the cost up there for your reference. I think it is a rough estimate. I put it in to have some way to illustrate the scale of the cost.

Next slide, please (slide 8).

LONG-TERM DEVELOPMENT AND UTILIZATION
PROGRAM OF THE ATOMIC ENERGY
AEC, JUNE 1982

--- VIGOROUSLY ADVANCE FUSION ENERGY DEVELOPMENT

--- ACHIEVE SELF-IGNITION AND ENGINEERING FEASIBILITY IN THE LATTER HALF OF 1990's IN FER

--- ADVANCE THE DEVELOPMENT OF FER, CONSIDERING IT AS A TOKAMAK

--- PROCEED WITH THE ALTERNATIVES WITH A LONGRANGE VIEW AND WILL MAKE JUDGEMENT IN COMPARISON WITH TOKAMAK WHEN THEIR POSSIBILITY OF ACHIEVING BREAKEVEN PLASMA CONDITION AND VIABILITY AS A REACTOR ARE ESTABLISHED.

SLIDE 8

This slide (slide 8) shows the long-term development and utilization program of the Atomic Energy Commission [AEC]. This plan was launched in June of this year by the Japanese AEC with a general description of the program. The fusion part of the program is to advance fusion energy development vigorously and also to achieve self-ignition and engineering feasibility in the latter half of the 1990's in the fusion engineering reactor [FER], I will explain the FER later in more detail.

This schedule shows that by the latter half of the 1990's, the FER should demonstrate engineering feasibility. So that means that completion of the reactor must be in 1991 or 1992. The schedule also shows the advanced development of FER, considering it as a Tokamak.

In this schedule, the AEC is going to select the best scheme when we will be in construction of FER.

As we all know, the Tokamak is the most advanced concept, so the whole development is systemized according to the Tokamak reactor.

The fourth item is mainly for the universities. That is, to proceed with the alternatives with a long-range view and make a judgment in comparison with Tokamak when their possibility of achieving plasma condition and viability as a reactor are established.

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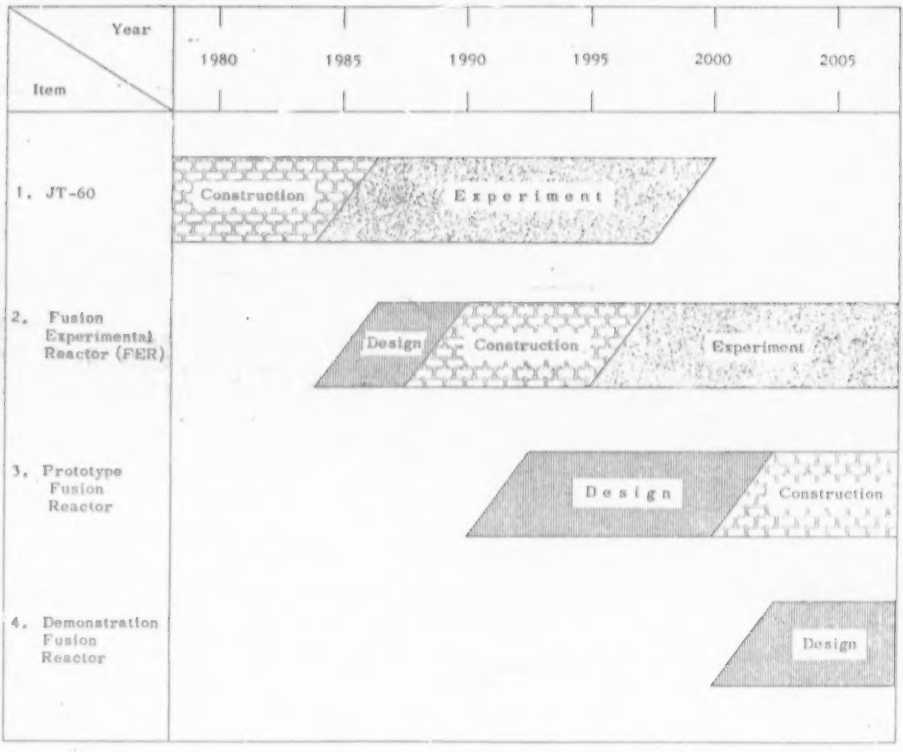


SLIDE 9

The schedule looks like this (slide 9). JT-60 will be completed in 1985. The FER will be completed in 1995.

We will also proceed with the prototype fusion reactor in 1995.

PHASES / DEVICES	PHYSICS	SCIENTIFIC FEASIBILITY	ENGINEERING FEASIBILITY
TOKAMAK (JAERI)	JFT-2 → R:0.9m, a:0.3m, B:1.6T; T:1.5keV, n:5×10^{13} cm^{-3}, τ:0.02s.	JT-60* → R:3m, a:0.95m, B:4.5T.	FER R:~5.4m, a:~1.5m, B:5-6T, 400-600 MW_{th}
(IPP)	R-Project (planning) R:2.1 m, a:0.6m, B:5T.		
STELLARATOR (Kyoto)	Heliotron E R:2.2m, a:0.2x0.4m, B:1.9T; T:0.9keV, n:5×10^{13} cm^{-3}, τ:0.05s.		assessment
MIRROR -tandem- (Tsukuba)	Gamma-10* 0.15-radius, 10m-long, B:2T.	assessment ---→	
LASER FUSION (Osaka)	Gekko-XII* laser power:20kJ, peak power:40TW, pulse width: 100ps-3ns.		

* : The devices where no data of plasma are shown, are under commissioning or construction OR DESIGN STUDY.
R: major radius, a: minor radius, B: magnetic field,
T: temperature of plasma, n: density of plasma,
τ: confinement time.

SLIDE 10

These are the stages of our research (slide 10). For the Tokamak, we have finished the physics stage and we are stepping into the scientific feasibility stages. After the operation of the JT-60, we need engineering experience and we will go to the experimental reactor. In addition there is also the stellarator Heliotron E at Kyoto University, the tandem mirror at Tsukoba University, and also we have a laser fusion activity in Osaka University.

FER REQUIREMENTS

1. SELF-IGNITION
2. D-T BURNING FOR ABOUT 130 SEC
3. DEFINITIVE DEMONSTRATION OF ESSENTIAL FUSION TECHNOLOGIES, SUCH AS SYSTEMS INTEGRATION AND HEATING, MAGNET, TRITIUM, AND PLASMA CONTROL TECHNOLOGIES
4. TEST AND TECHNICAL EXPERIENCE IN MATERIAL, TRITIUM PRODUCTION, REMOTE MAINTENANCE, RELIABILITY, DURABILITY, AND AVAILABILITY IMPROVEMENT.
5. TRITIUM BREEDING RATIO OF ABOUT UNITY
6. MODERATE WALL LOADING OF ABOUT 1 MW/M^2
7. POSSIBLE STAGED OPERATION SCENARIO TO BE EVALUATED

SLIDE 11

The next slide gives the FER requirements which include self-ignition, DT burning for about 100 seconds and definitive demonstration of essential fusion technologies such as systems integration and heating, magnet, tritium, and plasma control technologies.

The fourth item is for testing, that is test and technical experience in material, tritium production, remote maintenance, reliability, durability, and availability improvement.

Five, we want a tritium breeding ratio of about unity. This is one of the controlling factors of the design. Knowledge of the production of tritium is essential to continue the planning.

The sixth item is moderate wall loading of about 1 megawatt per square meter. This is like other designs such as the international design, which use 1.3 or so megawatts per square meter. The seventh item is a possible staged operation scenario yet to be evaluated.

This is the stated operation of FER. This means that it will operate and increase to full power over 2 or 3 years.

THREE TYPES OF FER DESIGN ARE
BEING STUDIED: REFERENCE, SWIMMING
POOL TYPE AND ADVANCED TYPE OPTIONS.

	FEATURES	HEATING	IMPURITY CONTROL
FER	OPEN-AIR	NBI & RF	DIVERTOR/PUMP LIMITER
FER-SP	SWIMMING-POOL	NBI & RF	DIVERTOR/PUMP LIMITER
FER-AD	OPEN-AIR CURRENT-DRIVE	RF	PUMP LIMITOR/DIVERTOR

SLIDE 12

We are studying three types of FER design (slide 12). The top one is open air. Next is the swimming pool type. This is a very special idea of the Japanese. We put all the Tokamak in the swimming pool and this eliminates the problems of neutrons between the seals, so that in the maintenance, we can have plenty of space for the machines. In general design, it appears to be good. The problem is that it takes a lot of time to pull out the concrete from the reactor.

Last is the advanced FER. That is the open-air current drive. This is the most advanced—an RF heated reactor.

FER PARAMETERS

MAJOR RADIUS	5.5 M
MINOR RADIUS	1.1 M
ELONGATION	1.5
PLASMA CURRENT	5.3 MA
TOROIDAL FIELD	5.7 T
HEATING POWER	60 MW
THERMONUCLEAR PR.	440 MW
BURN TIME	100 SEC
DUTY CYCLE	50 %
IMPURITY CONTROL	DIVERTOR/PUMP LIMITER
BREEDING MATERIAL	Li_2O
BREEDING RATIO	1.0

SLIDE 13

As shown, the FER has a major radius of 5.5 meters (slide 13). you can see the other numbers there. These are the FER parameters.

Thermonuclear power output is 440 megawatts. We have not yet decided the exact impurity control system. We are studying two versions, the divertor, and pump limiter.

I might say this is a little larger than the International Tokomak Reactor known as INTOR.

DEVELOPMENT PROGRAMS

HEATING

NBI DOUBLET III (10.8MW), JFT-2M (2MW)
JT-60 (20MW)

LHF JFT-2M(0.8MW), JT-60(27MW)

ICRFF JFT-2M(4MW), JT-60(6MW)

ECH DOUBLET III(2MW)

CURRENT DRIVE

LHF JFT-2M(0.8MW), JT-60(8.3MW)

FW JFT-2M(4MW)

MAGNETIC TECHNOLOGY

TFC CTF(8T-10T-12T)*, LCT(8T)

PFC SETF(20MJ)*

TRITIUM TECHNOLOGY

TPL(10^4 Ci), TEF*(10^6 Ci)

MATERIALS

STRUCTURAL HFIR/ORR**

BLANKET JMTR

1ST WALL SITF* (30kW E-BEAM)

NEUTRONICS FNS($5 \cdot 10^{12}$ N/SEC)

*PLANNED, ** UNDER DISCUSSION WITH US

SLIDE 14

The next slide shows the development programs for FER (slide 14). In heating, we have Doublet III and JFT-2M. Also, we are studying the current drive by LHF and some of the others. Please note carefully that in 1986 we are planning to perform a current drive experiment in JT-60.

In magnetic technology, both toroidal field coil [TFC] and poloidal field coil systems [PFC] are being developed. Recently we finished a large test coil in Japan and it will be transported to Oak Ridge by the end of November this year. This is a first large superconducting coil in the world. For the PFC system we are now planning a 20MJ test facility.

As to tritium, we are building a new building for that technology. We will establish fusion technology using 10,000 curies of tritium. Then we will go to the next step with 1 million curies.

With regard to materials, we are now discussing with the U.S. Government on the possible use of HFIR, and ORR under the Japan-United States Energy Cooperation Agreement, and this is in addition to the current program. Structural materials, of course, are the key point of the fusion reactor and we had a whole discussion of how to make sure that the construction materials are right in the fusion reactor.

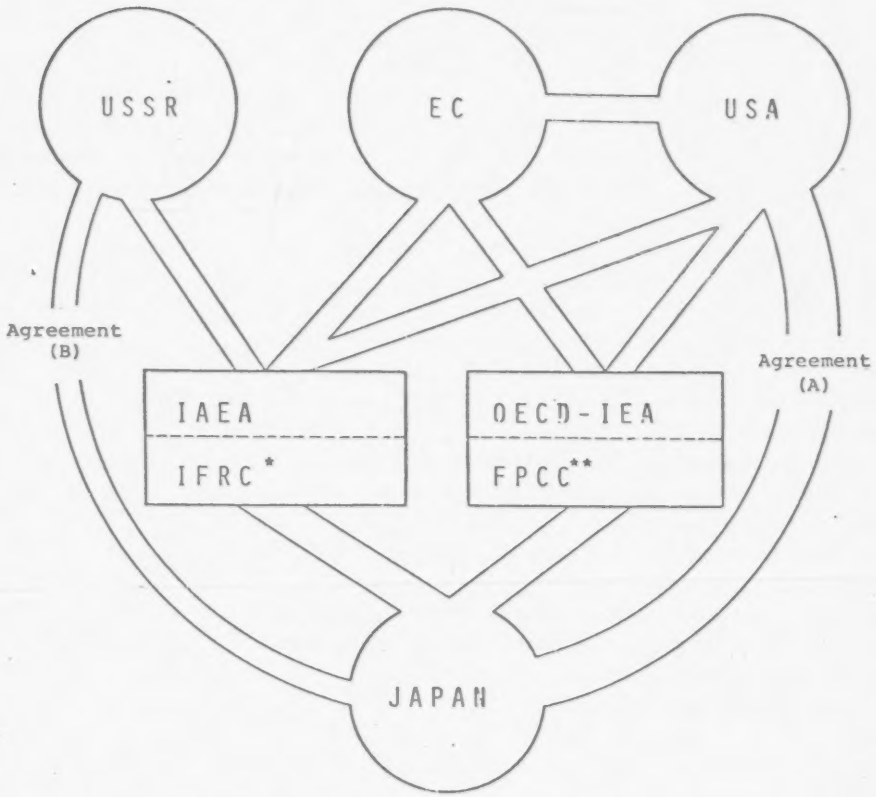
In the discussion, we say we need a test in HFIR, and at the same time we need a test using a source of 14-MeV neutrons. There are a lot of other ideas for equipment, but at present, the FMIT in the United States is the most advanced and I know that the project is very good.

So the Japanese scientists are very much interested in the FMIT project.

The blanket material will be tested in the JMTR, the test reactor in Japan.

In neutronics we have the FNS, with 5 times 10 to the 12th power neutrons per second, and this is under discussion with the United States. This is part of the conference. I think this is the most advanced device for experiment. Also I hope that the U.S. people will come to the FNS facility and make a joint experiment with us using this machine.

INTERNATIONAL COOPERATION
ON FUSION R&D



* International Fusion Research Council

** Fusion Power Coordinating Committee

Agreement (A) : Agreement between the Government of Japan and the Government of the United States of America on Cooperation in Research and Development in Energy and Related Fields. (May 2, 1979)

Agreement (B) : Agreement between the Government of Japan and the Government of the Union of Soviet Socialists Republic on Cooperation in Science and Technology. (October 10, 1973)

SLIDE 15

The last slide shows the major international cooperation on fusion R&D in Japan (slide 15). If you start from the left, we have the U.S.S.R. and Japan that have an agreement on cooperation and technology. It was started in 1973. We had no activity in fusion until recently. Some two years ago we had a conference and we will have another one. So this agreement is in force. We have the IAEA, of course, and the International Tokamak Reactor project and other IAEA activities. Also, we have agreement on materials testing and materials. We have a lot of programs under IEA under the direction of Dr. Palumbo.

That concludes my presentation.

Thank you.

Mr. RICHARDSON. Thank you very much, Dr. Mori, for bringing us this view of Japan's efforts. I would now like to encourage discussion around the table first from the panel itself and then from the observers.

Dr. WATSON. Let me ask, what do you consider to be the ultimate objective of the Japanese program? We had some discussion this morning on whether or not the objective should be to have a fusion generating device for electricity. Does Japan see that as the principal objective of their program?

Dr. MORI. Of course, yes. That is one of the major objectives. But as you know, the energy spent is only 30 percent of Japanese consumption of energy in electricity so we need to have other energy such as through nuclear. So we can produce synthetic fuels by fusion.

Dr. WATSON. Yes, thank you.

Dr. GOTTLIEB. He was asking whether the primary objective is power fusion or fuel production. What is the primary objective?

Dr. MORI. Hybrid production of synthetic fuel, you mean, or between fusion and fission?

Dr. WATSON. Yes.

Dr. MORI. Well, there is some controversial discussion on that point. The Japanese people are intent on safety from the environmental point of view, so power fusion is the top priority of our aim.

Mr. RICHARDSON. Bob?

Dr. SCOTT. You have listed the FER reactor as part of a planning exercise. How do you see that fitting into your develoment program?

Dr. MORI. As you may know, the FER device is not a reactor.

Dr. SCOTT. Well, a device.

Dr. MORI. The 1.3 is the highest Q value. It is used for plasma physics.

Dr. SCOTT. But it is a DT system, I understand?

Dr. MORI. Yes. We will use that and other technology will be gained.

Mr. RICHARDSON. Dr. Mori, in Japan, is the development of fusion energy seen as an essential component of future energy supply in Japan?

Dr. MORI. It is very difficult to answer.

The people in the House of Representatives are very interested in fusion and they are personally supporting fusion.

The reason is that they studied it extensively and they found out that fusion is one of the most promising ways to solve the energy problems in Japan

Dr. GOTTLIEB. Could you give us some idea of the rate of increase of support for fusion in the last 10 years? How much of the expenditures have grown per year roughly?

Dr. MORI. Ten times. Increased approximately 10 times.

Dr. GOTTLIEB. I Would like to comment on that because I have been following the Japanese program and it has just been remarkable to me how the Japanese decided to push fusion and building on a very small base have now one of the really major programs in the world in a period of only 10 years. That is a remarkable achievement for Japan and for Dr. Mori himself, who has been the principal mover in this effort.

Dr. CLARKE. Could I ask a question?

Mr. RICHARDSON. Sure.

Dr. CLARKE. Dr. Mori, could you say a few words about the meaning of the national project for fusion? We don't have national projects in this country very often and it came up in our discussions this morning. I think we would like to hear about it.

Dr. MORI. Some projects receive special emphasis among AEC programs. The Japanese AEC decides to complete a plan on a project. It means time, money in the budget and other logistics, and they make a formal decision on the program which is approved by the Minister of Finance, but the approval is that if the Minister of Finance looks at the program and says nothing, that means it passes.

Dr. GOTTLIEB. It is a policy statement, is it not?

Dr. MORI. The policy statement is performed by the AEC. But on the money budgetary side, the AEC has to show the program beforehand to the Minister of Finance, and they will—they have to get some approval to the budget. This is why we can increase the budget—I showed the slide on the fourth viewgraph.

[Slide No. 4 put up.]

Dr. GOTTLIEB. The Minister of Finance did not say no and that is as much as you can expect, I think, in any country.

Dr. MORI. Oh, yes, but I don't know about all countries.

You see in 1975, it was about 5 billion yen, $20 million.

In 1975, that is the beginning of the so-called national project.

At present, the national projects are in fusion and the others a fast breeder reactor, and uranium enrichment; those three are the so-called special R&D applications, top priority projects.

Mr. RICHARDSON. Dr. Mori, let me ask you one other question. Does the general population in Japan understand the fusion program well enough to support it heavily? Let me ask, is public opinion well behind the fusion program?

Dr. MORI. If we observe from the newspapers, they were supportive to fusion some years ago, but recently expenditures increased so much that some people are concerned about the budget in fusion, especially the people in the Ministry of Education. In some instances, there is some antifusion opinion appearing in the newspapers. But in general, they are very supportive to the fusion program.

Mr. RICHARDSON. Of course the reason I asked was that in the United States, I think public opinion does not understand the fusion program as thoroughly as perhaps in your country.

Dr. MORI. You have a lot of energy.

Mr. RICHARDSON. Any further questions?

Mr. REICHLE. On this question that Dr. Clarke raised about a national program and what the significance of that is, my understanding is that once the Japanese program was made an actual national program, it reports to the Prime Minister and, therefore, is not subject to the budget cycle as much as normal programs, is that true?

Dr. MORI. Yes. Years ago, the fast breeder development was nominated as a national project. At that time it was reported to the Cabinet and the reasons are that they have to establish a new

program in the system and also the budgeting. I think that is why we got into that.

Dr. SCOTT. It that true for fusion?

Dr. MORI. No.

Dr. ROCKWOOD. Yes, Dr. Mori, I wonder if you could say a bit more about the Japanese view of your inertial fusion program at Osaka University, how that is going.

Dr. MORI. The laser fusion work is now in the testing stage. They are working to demonstrate feasibilty.

Mr. RICHARDSON. Thank you very much, Dr. Mori, for responding to our questions and for your presentation.

We would now like to move to our other guest from overseas, Dr. Donato Palumbo, Directorate General for Research, Science and Education of the Commission of European Communities.

Dr. Palumbo was at the same meeting in Baltimore that Dr. Mori has attended. We are fortunate to have you with us today.

STATEMENT OF DR. DONATO PALUMBO, DIRECTOR FOR RESEARCH, SCIENCE AND EDUCATION, COMMISSION OF EUROPEAN COMMUNITIES

Dr. PALUMBO. Mr. Chairman, I am director for fusion program inside the Directorate General.

Mr. RICHARDSON. Thank you for making that distinction. Dr. Palumbo, we are very comfortable for time. You may have all the time that you wish to make your presentation and to answer questions.

Dr. PALUMBO. Thank you. First of all, I should like to thank you for your kind attention. Second, I would say that while I am of the European Community, in some sense what I will be saying about fusion, I am also speaking a little for myself.

THE E.C. FUSION PROGRAMME

SUMMARY

INTRODUCTION

I. CONTENT OF THE PROGRAMME :
PRESENT STATUS, MEDIUM TERM PLANS, NET, TECHNOLOGY.

II. ORGANISATION OF THE COMMUNITY PROGRAMME :
COMMUNITY NATURE OF THE PROGRAMME, ASSOCIATIONS,
MANAGEMENT STRUCTURE, THE JET JOINT UNDERTAKING,
DECISION MAKING PROCEDURE, FINANCIAL VOLUME.

III. INTERNATIONAL COLLABORATION :
THROUGH THE IAEA : INTOR
THROUGH THE IEA : IMPLEMENTING AGREEMENTS ON TEXTOR,
LARGE COIL PROJECT, MATERIALS AND FMIT,
REVERSED FIELD PINCH AND RFX.
POSSIBLE BILATERAL COOPERATION :
ALTERNATIVE LINES (MIRRORS, STELLARATORS..)
TOKAMAKS AND HEATING
NEXT STEP AND RELATED TECHNOLOGY.

SLIDE 1

I should like to go into the technical program (slide 1), the organization of the Community program, and spend some time on the international cooperation we have between the agencies, and the existing bilateral cooperation, mainly with the United States.

THE PROGRAMME STARTED IN 1959.

IMPLEMENTED BY MEANS OF ASSOCIATIONS BETWEEN EURATOM AND THE MAIN NATIONAL INSTITUTIONS.

INERTIAL CONFINEMENT ABOUT 1 %.

MAGNETIC CONFINEMENT : MAINLY TOKAMAK.

TWO ALTERNATIVE LINES.

DIAGNOSTICS AND PLASMA HEATING (NEUTRAL INJECTION AND RADIO FREQUENCY.

TECHNOLOGY : SUPRACONDUCTING MAGNETS
TRITIUM SYSTEMS
BLANKET
REMOTE HANDLING
MATERIALS

SLIDE 2

Our program, started in 1959 (slide 2), is centered on the magnetic confinement and mainly on the Tokamak. Until a few years ago, the program was a plasma physics program; 2 or 3 years ago we determined the physics was already sufficiently advanced to devote more attention to the technology. Before taking this amplification of our program to include the technology, the Commission proposed to constitute a very high-level panel. So it constituted a panel chaired by Professor Beckurts, composed of very high-level European scientists, which reviewed the status of the fusion program, the prospect of fusion and the fusion plants, and formulated recommendations.

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TERMS OF REFERENCE

REVIEW : . STATUS OF THE FUSION PROGRAMME
. PROSPECTS OF FUSION
. FUTURE PLANS

FORMULATE RECOMMENDATIONS ON FUTURE POLICY AND ACTION.

FINDINGS AND RECOMMENDATIONS

NUCLEAR FUSION HOLDS GREAT PROMISE AS ONE OF THE NEW PRACTICALLY INEXHAUSTIBLE SOURCES WHICH MUST BE DEVELOPED IN ORDER TO SOLVE IN THE LONG TERM THE VERY SERIOUS ENERGY PROBLEMS OF EUROPE.

THE PANEL IS CONVINCED THAT EUROPE MUST REMAIN IN THE FRONT LINE OF FUSION R & D.

EFFORTS TO DEMONSTRATE SCIENTIFIC FEASIBILITY MUST CONTINUE AND NEW EFFORTS TO DEMONSTRATE TECHNICAL FEASIBILITY MUST BE STARTED.

SOME GROWTH IN REAL EXPENDITURE FOR THE EUROPEAN FUSION PROGRAMME IS REQUIRED.

THE PANEL PLACES PARTICULAR EMPHASIS ON THE SUPPORT NECESSARY FOR JET AND FOR THE START UP OF THE NET-TECHNOLOGY PROGRAMME AND RECOMMENDS THAT EFFORTS ARE MADE TO ATTRACT INDUSTRY PARTICIPATION IN THIS PROGRAMME AT AN EARLY STAGE.

WHENEVER POSSIBLE AND FEASIBLE, INDUSTRY PARTICIPATION IN THE DESIGN AND DEVELOPMENT OF ADVANCED EQUIPMENT SHOULD BE SOUGHT.

THE COMMUNITY PROGRAMME MUST BE REGARDED AS A PART OF AND A CONTRIBUTION TOWARDS WORLD FUSION KNOWLEDGE AND TECHNOLOGY.

SLIDE 3

The main findings of the panel were that nuclear fusion holds great promise as one of the new practical energy sources which could be used in Europe (slide 3). The panel is convinced that Europe must remain in the frontline of the fusion research and development. The efforts to demonstrate scientific feasibility must continue and new efforts to demonstrate technical feasibility must be started. Some growth in real expenditure for the European fusion program is required. The panel places particular emphasis on the support necessary for JET, for the startup of the NET technology program, and recommends that efforts are made to attract industry participation in this program at an early stage. Whenever

possible, and feasible, industry participation in the design and development of advanced equipment should be sought.

The Community program must be regarded as a part of a contribution toward world fusion knowledge and technology. As I said, our program is centered on the Tokamak—next, please.

TOKAMAKS

TWOFOLD INTEREST :
- POSSIBLE BASIS FOR REACTOR OR AT LEAST FOR THE NEXT STEP
- TOOL FOR PLASMA PRODUCTION FOR THE DEVELOPMENT OF HEATING, DIAGNOSTICS, REFUELLING ETC.

MAIN TOKAMAKS

First operation	Device	Main objectives (present or future)	Plasma current (kA)	Pulse duration (sec)
1973	TFR (Fontenay)	Plasma heating, physics	400/600	1
1976	DITE (Culham)	Divertor, heating, physics	250/350	0.5
1977	FT (Frascati)	Scaling laws, RF-heating	1000	1
1980	ASDEX (Garching)	Divertor, heating, refuelling	500	5
1981	TEXTOR (Jülich)	Plasma-wall interaction	500/600	3

MAIN RESULTS

TFR : BROAD COMPREHENSION OF CONFINEMENT AND HEATING
DITE : NON-INDUCTIVE CURRENT DRIVE
FT : RECORD PERFORMANCE ON
ASDEX : LONG PULSE AND GOOD DIVERTOR PERFORMANCE

SLIDE 4

We consider the Tokamak with a twofold interest (slide 4). First, there is the possible basis for reactor or at least, for the next step. Second, they represent a tool for producing plasma for the development of heating, diagnostics, refueling, et cetera.

The table has the details of our important resources which contribute adequately to the two aspects of Tokamak interest.

SMALLER TOKAMAKS

Device	Site	Main objectives	I (KA)	t (sec)
PETULA	Grenoble	RF heating (lower hybrid)	100	0.1
ERASMUS	Brussels	RF heating (ion cyclotron)	50	0.015
TCA	Lausanne	RF heating (Alfven waves)	130	0.1
WEGA	Grenoble	RF heating (lower hybrid	100	0.1
TORTURE	Jutphaas	Turbulent heating	100	0.005
THOR	Milan	RF heating (electron cyclotron)	55	0.01
RINGBOOG	Jutphaas	Cold mantle between plasma and wall	70	0.1
DANTE	Risø	Pellet ablation for refuelling	20	0.05
TOSCA	Culham	High beta, RF heating (electron cyclotron)	25	0.005
SPICA	Jutphaas	High beta	1000	0.001

SLIDE 5

The next viewgraph (slide 5) is devoted to a set of small Tokamaks which are in different laboratories for particular objectives. The main Tokamaks are the rather large or medium-size Tokamaks which have the aim to develop the physics in order to improve the performance. The smaller Tokamaks are used mainly for other specific purposes. For instance, the Grenoble WEGA Tokamak is mainly devoted to RF heating.

The JET Project

OBJECTIVE:

The objective of JET is to obtain and study a plasma in conditions and with dimensions which approach those needed in a thermonuclear fusion reactor.

STUDIES:

The experimental studies will be aimed at defining the parameters, the size and the working conditions of a tokamak fusion reactor. These studies are:

The study of plasma processes and scaling laws into regions close to those needed in a thermonuclear fusion reactor.

The study of the interaction of the plasma with the torus walls. This interaction is important because it controls the purity of the plasma which in turn determines the energy loss by radiation.

The study of methods of heating the plasma to temperatures at which the energy released by fusion reactions would sustain the plasma.

The study of α-particle production, confinement, and consequent plasma heating.

SLIDE 6

Our major project is JET (slide 6). JET is the largest Tokamak under construction in the world, with a horizontal minor radius of 1.25 meters. The vertical radius is 2.1 meters. It has an elongated cross-section. The magnetic field will be of the order of 35 kilogauss. The machine will be capable of handling about 5 megawatts input heating power.

We hope that the machine could be started in operation in the middle 1980's, with initial operation beginning in June or July of the next year.

The main objectives of JET are to start a plasma and then produce and study the plasma in conditions which approach those needed in a fusion reactor.

PHASES IN THE OPERATION OF JET

Phase	Planning Date	Effective Heating Power (in addition to Ohmic)	Description
I	mid 1983	0	Initial operation and commissioning with ohmic heating only.
II A	mid 1984	5 MW	Operation with Neutral Injection (N.I.) Heating 5 NW of 80 keV(H).
II B	Jan 1985	16 MW	Additional Heating level: 10 MW; 80 keV(H) N.I. + 6 MW (effective) ICRH
III A	Jan 1986	19 MW	10 MW: 80 KeV(H) or 160 KeV(D) of N.I. + 9 MW (effective ICRH).
III B	mid 1987	25 MW	10 MW; 160 KeV(D) of N.I. + 15 MW (effective) ICRH.
IV	mid 1988	25 MW	Operation with significant α-particle heating in deuterium-tritium gas mixtures.

SLIDE 7

We have several phases (slide 7). Phase 1, which could start immediately, is the initial operation and commissioning with the machine. We will reach full operation in this phase by 1987 or so. If we get high enough temperature, long enough confinement time, and enough density then at that moment we will decide if we could inject a measure of tritium; this is a very critical decision because after a few months of operation, if the operation is good enough, the machine will be out of order. It will then be impossible to run this machine. So this will be a crucial time.

NEW TOKAMAKS

	Objectives	Present status	Envisaged operation date
TORE SUPRA (Cadarache)	Superconducting coils – Long pulse physics – Heating – Impurity diffusion study	under construction	1986
FTU (Frascati)	Scaling laws (high $n\tau$) Lower Hybrid Resonance Heating	Formal proposal	1985
ASDEX-UPGRADE (Garching)	Poloidal divertor outside toroidal field coils – Cold plasma mantle – Impurity diffusion control – Non-inductive current drive.	Definition phase	1988

OPEN PROBLEMS

- BETA LIMITS, DISRUPTIONS...
- BURNING PLASMA AND IGNITION : HOPEFULLY SOME INFORMATION FROM TFTR AND JET, POSSIBLY FROM A VERY HIGH FIELD TOKAMAK UNDER REVIEW

HEATING :

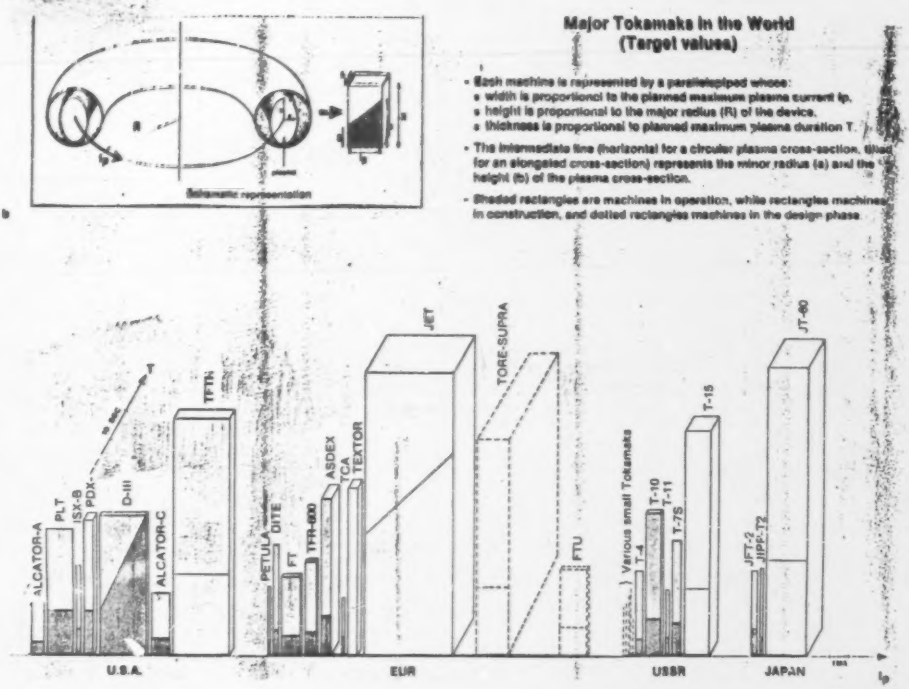
- NEUTRAL INJECTION : FONTENAY AND CULHAM
  ALFVEN WAVES : LAUSANNE
  ION CYCLOTRON : FONTENAY AND BRUSSELS
  LOWER HYBRID : GRENOBLE
  ELECTRON CYCLOTRON : STARTING IN VARIOUS LABORATORIES

SLIDE 8

I repeat, JET is the only Tokamak which has exactly the size of the machine of the next generation as they are conceived now. So it will be very important to have almost full-scale test of the new generation on JET. We have quite a number of new Tokamaks in the planning process (slide 3). But only one is under construction. For the remaining devices, no decision has been taken.

In France, the Tore Supra is under construction. It is a very large Tokamak. But what is more important, it is a superconducting Tokamak. This will be a very important tool for the European program and perhaps for the Western world program, a unique machine of this kind.

Well, in addition we know that the Tokamaks at the moment have not solved all the problems. We are dealing with these problems.



SLIDE 9

The next slide (slide 9), which is perhaps in too much detail, shall give a panorama of the Tokamak situation, worldwide. The volume of the box is in proportion to the size, geometrical size of the machine, to the duration of the pulse and roughly to the magnetic field. Well, you see that, as I said, our program is almost all Tokamaks, so it is not surprising if the presence of Europe in this graph is important. On the left are the Tokamaks in the United States, followed by Japan and the Soviet Union. You see a very big volume in JET. This is due to the fact that the bursts will be very long because it is a superconducting machine.

ALTERNATIVE LINES

STELLARATOR

ADVANTAGES : CONTINUOUS OPERATION, HOPEFULLY HIGHER BETA

IN OPERATION : W VII A (GARCHING)
- CONFINEMENT COMPARABLE OR BETTER THAN EQUIVALENT TOKAMAK
- HEATING 1.2 MW NI AVAILABLE
  3 MW ICRH PLANNED

UNDER DESIGN : W VII AS (GARCHING)
- LARGER
- MODULAR TWISTED COIL SYSTEM

REVERSED FIELD PINCH

ADVANTAGES : HIGH BETA, OHMIC HEATING

IN OPERATION : HBTX 1A (CULHAM), ETA BETA II (PADUA)
COMPARABLE TO ZT 40 (LOS ALAMOS)

DESIGN AVAILABLE : RFX (2 MA), JOINTLY DESIGNED BY CULHAM, PADUA AND LOS ALAMOS.
CONSTRUCTION AT CULHAM NOW CANCELLED FOR BUDGET REASONS.
POSSIBILITY TO BUILD IT AT PADUA UNDER REVIEW.

SLIDE 10

In addition to the Tokamak we have two alternative lines. One is the stellarator (slide 10). This is in the Federal Republic of Germany. Its possible advantages are continuous operation, and hopefully higher beta, which are not yet assured. In operation in Garching, there is confinement comparable or even better than an equivalent Tokamak, using neutral beam heating of 1.2 megawatt. More heating is planned.

The new stellarator is under design in Garching and I think construction will begin before end of the year. The characteristic of the machine will be to have a modular twisted coil system which perhaps could make the stellarator a little more suitable for reactor than was thought in the past.

The second alternative line we have is the reversed field pinch, which has the advantage of ohmic heating. We have two devices. These machines are comparable to the one in Los Alamos. We have under design a larger machine which has a capacity of 2 Mega-

amps This has been jointly designed by Padua, Culham and Los Alamos. Eight months ago they canceled the approach for various reasons. At the moment we are considering the possibility of building this machine. A final decision should be taken by the end of the year.

Thus, for the long-term plan, our aim is officially decided by the Council of Ministers. The aim of the European fusion program is the joint construction of a prototype fusion reactor which coincides with what you call DEMO.

LONG AND MEDIUM TERM PLANS

OFFICIAL AIM OF THE PROGRAMME : JOINT CONSTRUCTION OF A PROTOTYPE FUSION REACTOR (DEMO).

DEMO DEFINED AS REACTOR WITH A NET ELECTRICITY PRODUCTION NOT NECESSARILY AT COMPETITIVE COST.

THE GAP BETWEEN JET AND DEMO TO BE FILLED BY A SINGLE INTERMEDIATE STAGE (NEXT STEP) WHICH SHOULD :

A) RESOLVE THE REMAINING SCIENTIFIC PROBLEMS ;
B) INCORPORATE SOME OF THE TECHNOLOGICAL COMPONENTS OF DEMO ; AND
C) ACT AS A TEST BED FOR OTHER TECHNOLOGICAL COMPONENTS.

DECISION MADE TO CONSTITUTE A NET (NEXT EUROPEAN TORUS) TEAM WITH THE TASK OF :

. PRODUCING A CONCEPTUAL DESIGN BEFORE THE END OF 1984
. CONTRIBUTING TO INTOR AND/OR SIMILAR INTERNATIONAL VENTURES
. PROVIDE FOCUS AND GUIDELINES FOR TECHNOLOGY PROGRAMME.

THIS IN ACCORDANCE WITH THE PANEL RECOMMENDATION.

SLIDE 11

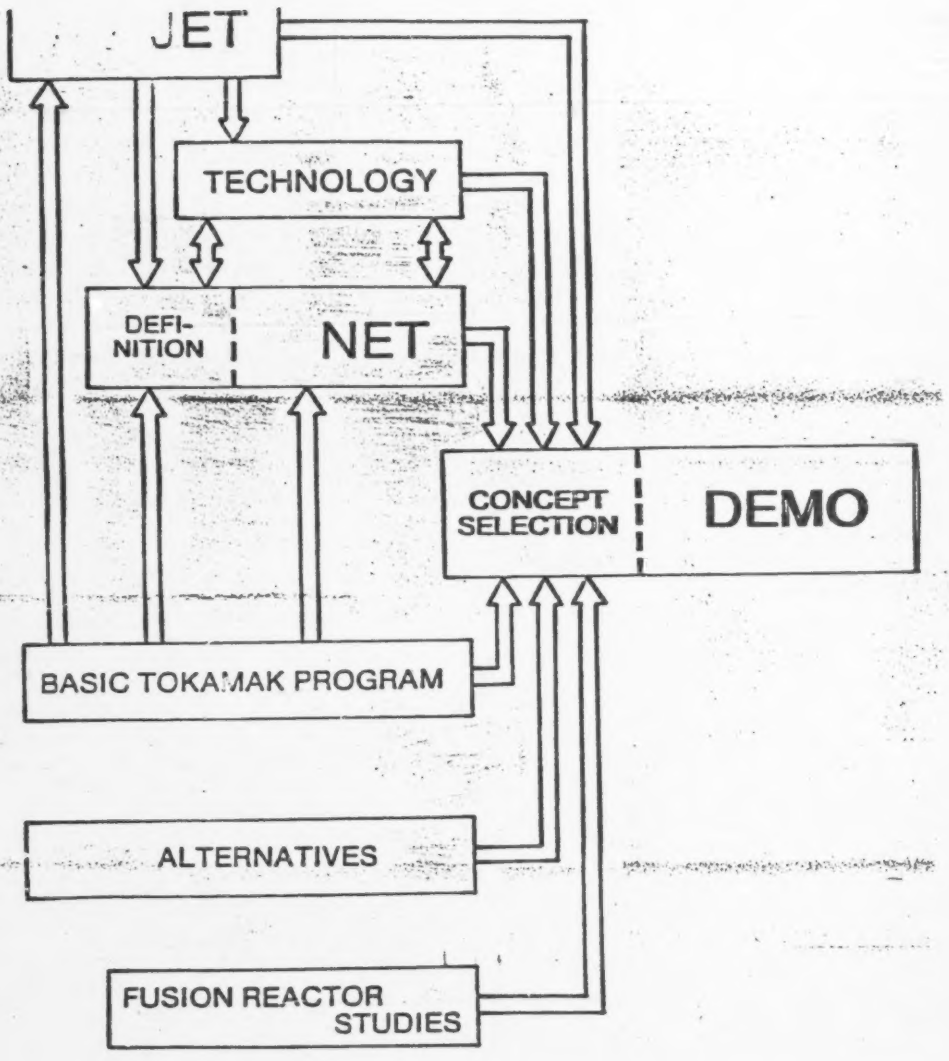
This gap could be filled by single intermediate stage which we call the next stage (slide 11). The machine we call NET which we feel is a single, intermediate stage. The mission of NET should be to solve the remaining scientific problems. For instance, the physics incorporates some of the technological components of DEMO and act as a test bed for other technological components. This decision has been taken not only at the level of my colleagues but by the Council. This is part of a 5-year program which has been approved this year by the Council. So we have already constituted a

NET team. This is not official but not a secret; this team has the task to produce a conceptual design before the end of 1984.

This is fully in accordance with the panel recommendation shown at the beginning. Perhaps I would like to say a few words about the last line, "provide focus and guidelines for technology program." As I said, in the past our technology program was rather weak. Now we have decided to increase this activity. I think this activity should be increased having in view some more or less precise objective. NET should provide the objective. The task should distribute among the laboratories the task which will be necessary for the construction.

The construction of NET is viewed as a single European venture, because in Europe, many people should prefer to have this next step as a common venture between several program. But if we go ahead with NET as a single European venture, we think that the real construction would start by the end of this decade?

Dr. GOTTLIEB. End of this decade.



THE RISK OF THE PROVISIONAL CHOICE OF THE TOKAMAK SYSTEM FOR NET CAN BE AFFORDED SINCE TECHNOLOGY IS IN COMMON WITH OTHER MAGNETIC CONFINEMENT SCHEMES.

NET CONSTRUCTION COULD START END OF THE 80's.

SLIDE 12

Dr. PALUMBO. Our strategy is illustrated in this slide (slide 12). NET, which you see in the center of the slide, will be the result of our Tokamak program. Construction will start after the results of JET, with the input of the technology which has now started, thereby providing the facility with which to eliminate the uncertainties of Tokamaks. So the definition process on the left is al-

ready started. For us the DEMO concept selection will be between NET and alternatives—if there is some disaster in the physics this decision can be changed, but for the moment we assume NET will be a Tokamak. We think that the risk of the choice of the Tokamak system for NET can be afforded since technology is in common with other confinement schemes.

In the fusion technology we have activities making a program that is more or less similar to what Dr. Mori said for Japan. I shall go fast through this. Let me go now to the second part of my talk. I shall try to go very fast.

ORGANISATION OF THE COMMUNITY FUSION PROGRAMME

BY DECISION OF THE EUROPEAN COUNCIL OF MINISTERS THE PROGRAMME "EMBRACES ALL WORK CARRIED OUT IN THE MEMBER STATES IN THE FIELD OF THERMONUCLEAR FUSION. IT IS DESIGNED TO LEAD IN DUE COURSE TO THE JOINT CONSTRUCTION OF A PROTOTYPE REACTOR WITH A VIEW TO ITS INDUSTRIAL PRODUCTION AND MARKETING."

IMPLEMENTED BY MEANS OF ASSOCIATION CONTRACTS BETWEEN EURATOM AND NATIONAL ORGANISATIONS OF THE MEMBER STATES AND OF SWEDEN AND SWITZERLAND WHICH ARE FULLY ASSOCIATED TO THE PROGRAMME.

FINANCIAL CONTRIBUTION :	RUNNING COSTS	25 %
	CAPITAL INVESTMENT	45 %

JET : JOINT UNDERTAKING (LARGE FINANCIAL AND MANAGERIAL AUTONOMY)

FINANCING :	EURATOM	80 %
	UKAEA	10 %
	ALL THE ASSOCIATED LABORATORIES	10 %

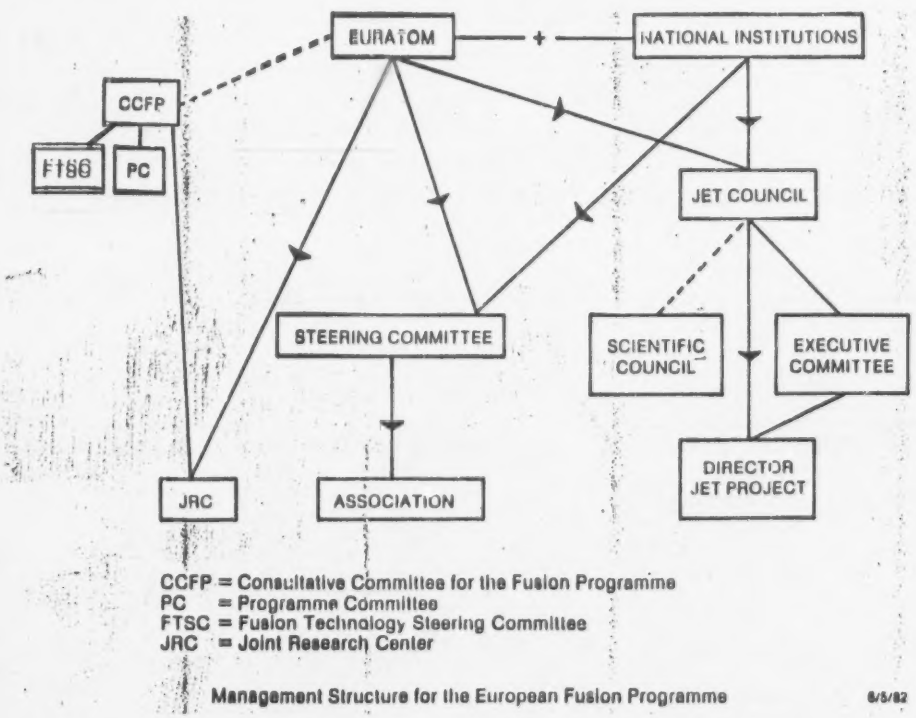
SLIDE 13

I would like to turn now to the organization of the fusion program (slide 13). By decision of the European Council of Ministers, the program embraces all work carried out in the member states in the field of thermonuclear fusion. It is designed to lead in due course to the joint construction of a prototype as I stated before. So an important characteristic of this program, I think it is the only scientific program in Europe which has this characteristic, is that all the activity of the member states, everything done in France,

Germany, et cetera, is implemented by means of association contracts between Euratom and national organization of the member states, and also of Sweden and Switzerland. These two countries are not members of Euratom, but for the fusion program they ask to be considered as equivalent to a member with the same obligation and rights.

Euratom contributes to the costs of each national organization; specifically, 25 percent for the running costs and 45 percent for the capital investment. For JET, which is a particular joint undertaking, the Commission pays 80 percent of the total cost. The United Kingdom pays 10 percent as a special contribution, and all the other associated laboratories, French, Germans, Italians, et cetera, pay the remaining 10 percent. I think this is important because I insist very much at this point that JET has to be felt by all European laboratories as a common facility and common tool. The geographical distribution of the laboratories is on this map.

I think it is impossible to look at all the laboratories because some of them are small with only a few tens of scientists. Some of them have 1,000 persons, out of whom there are 100, 200, or 250 professional.



SLIDE 14

The next viewgraph shows the organization of the fusion program in Europe (slide 14). Funds come from Euratom and national institutions the United Kingdom Atomic Energy Authority, et cetera. At the top of the program, implementation is made by the

Fusion Directorate in Brussels, by my office. We have a committee, CCPF, which is the main committee for the management of the program. This committee is constituted by chief members for each member state, appointed by their colleagues. There are two subcommittees: the Fusion Technologies Steering Committee, which has some degree of power, in particular to NET: and the Program Committee, which is mainly for the physical part of the program.

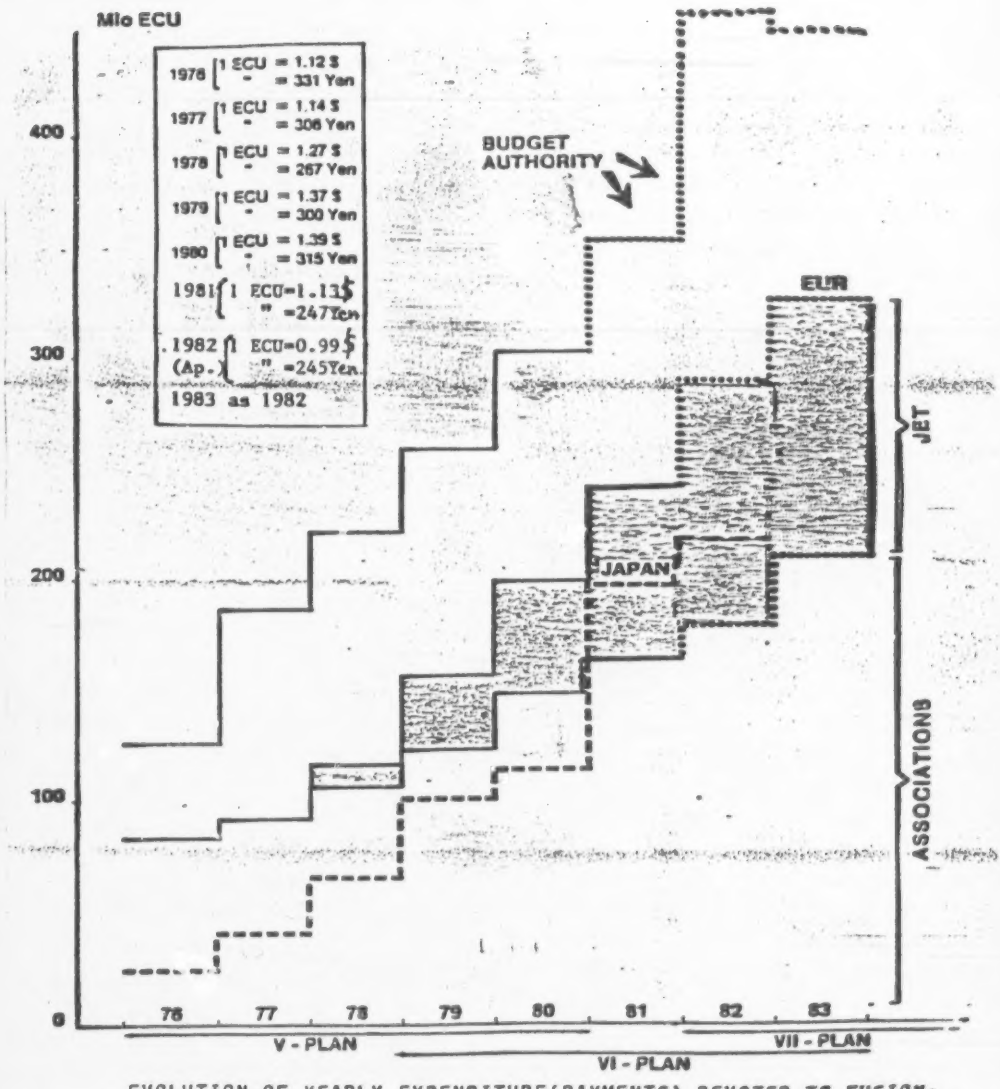
Each association, for instance, the association between the Euratom and France, has a steering committee composed of members from Euratom and members appointed by the national organization. JET, on the right, is a sort of autonomous association. This association meets twice or three times per year and assists the Council, which is meeting six times per year, and the director, Dr. Wüster, and assisted by Dr. Rebut, and the father of JET, who is the director responsible for construction. Dr. Wüster is director of the overall Jet Joint Workshop.

All this works rather well. It is less complicated than could be imagined. My main job is the preparation of the 5-years' program. The program is such that it is designed for 5 years, but after 3 years, there is a new 5-year program which is overlapping. So my major job is preparation of this program. We start in consultation with the laboratories; this takes a few months.

There are tasks of 20 years or so. We prepare a draft, and we discuss the draft inside the CCFP, two or three times. In this case CCFP meets every month. After reaching an agreement, this is endorsed by the Commission, and submitted to the European partners for discussion and to the Council of Ministers for final decision. All that takes about 1 year, roughly.

The fact that we have a 5-year program gives to the program some stability. We have money for 5 years. Now, I know how much I have for the next 5 years at least. I can get more, but the money which has been given cannot be reduced.

Dr. GOTTLIEB. I hope everybody heard that one.



EVOLUTION OF YEARLY EXPENDITURE(PAYMENTS) DEVOTED TO FUSION:
USA:magnetic confinement programme(excluding inertial,including mirrors)
JAPAN:overall programme(including inertial)
EUR: overall programme(including Sweden,Switzerland an JRC)
5 year programme 1982-86:
Commission'appropriation of 620 Mio ECU (JET + Associations), which corresponds to about 1.4 Bio ECU expenditure.

SLIDE 15

Dr. PALUMBO. OK. Well, the next slide (slide 15) gives the evolution of the European commitment, the European budget in comparison with Japan and United States. Don't think this too literally because it depends on the ratio between the currencies. Our cur-

rency, European, which until recently was about $1.10 to $1.39 is now 99 cents. I don't know if it has changed in the meantime. So don't consider this as absolutely exact.

Our program is a bit larger than the Japanese program which is increasing enormously having started from zero in 1978 and now being almost equal to the European program; the Japanese program is considerably smaller than your program.

That concludes my presentation of the European program. Now I should devote, if you agree, Mr. Chairman, 10 minutes to the international cooperation we have.

INTERNATIONAL COOPERATION

FUSION IDEAL AREA FOR INTERNATIONAL COOPERATION MOTIVATED BY :

- UNIVERSAL ADVANTAGES OF POSSIBLE SUCCESS
- DIFFICULTY OF SCIENTIFIC AND TECHNOLOGICAL PROBLEMS

COST OF THE 20-YEAR PROGRAMME UP TO DEMO : ABOUT 20 BILLIONS, WOULD REQUIRE TRIPLING THE YEARLY EXPENDITURE OF EACH OF THE LARGE PROGRAMMES.

SAME RESULT OBTAINABLE BY ADDITION OF PRESENT LEVEL PROGRAMMES : UTOPIC BUT ILLUMINATING.

ALL ALTERNATIVE LINES CANNOT BE FULLY DEVELOPED BY EACH OF THE LARGE PROGRAMMES.

COOPERATION IN THE IAEA FRAME : INTOR

PHASE ZERO	: DATA BASE ASSESSMENT)	
	)	USEFUL
PHASE ONE	: DEFINITION)	
PHASE TWO A	: DESIGN WORKSHOP IN PROGRESS	
FUTURE	: $0 \leq P_{INT} \ll 1$	

SLIDE 16

From the beginning fusion has been considered as an ideal subject for international cooperation (slide 16). This is motivated first

by the universal advantages of possible success of fusion programs, and second, by the enormous difficulties, scientific and technological, and I should add also those concerning finances and staff.

Let me add two precise points, two examples. Both the United States and Europe are making long-term plans including cost estimates. Let me say 20 years from now the cost will be about $20 billion.

If you refer to the previous slide this would require from our side a significant multiplication of funding. But if this could be made by an additional international cooperation and pooling of resources this level could be made without an increase of burden, because already we are at the level of expenditure of about $1 billion per year. I know that this view is myopic, naive, perhaps, but it does show the direction where we should go step by step.

The second is the issue of alternative systems. As I say, 80 percent of our program is in Tokamaks; it is not exactly the same, but more or less true for the other programs. In order to bring the Tokamak to the present level, which is good and I think I should say even after this week in Baltimore, this conference, is very good, the Soviet Union, United States, Japan, and we have spent billions of dollars. Thousands of scientists have worked for 20 years of more. Can we imagine that for each of the remaining concepts each program could spend a comparable effort? I think it is a necessity for the alternative lines to have some sharing between the dimension of work program in order to bring these up to the point where they can be compared.

Well, excuse me for this philosophical consideration. Let me go to the facts. First, there is an official cooperation through the International Atomic Energy Agency. The international exercise called INTOR has been under chairmanship of my Japanese colleague, Dr. Mori, who has been chairman of the group from the beginning. Until now this work has been useful, very useful and very successful.

But now the problem is, is it realistic to think that in a reasonable time we can go to the initial aim of this project, joint construction and operation of this machine as a four-partner venture, Europe, Japan, United States, and the Soviet Union? Today I think in Washington, perhaps also in Brussels, diplomatically this event appears as usual, between zero and one, but much more near to zero than to one. This is something which we should consider seriously.

COOPERATION IN THE IEA FRAME

TEXTOR (US, EUR, JAPAN ET AL.) AT JÜLICH

- . PLASMA-WALL INTERACTION DEDICATED TOKAMAK.
- . IN OPERATION.
- . STAFF PARTICIPATION IN DESIGN, CONSTRUCTION AND OPERATION.
- . EXCELLENT COOPERATION BEYOND LEGAL COMMITMENT AND EXPECTATION.

- SUPERCONDUCTING COILS (US, EUR, SWITZERLAND, JAPAN) AT OAK RIDGE

- . TOROIDAL ASSEMBLY OF 6 LARGE SUPERCONDUCTING COILS (3 AMERICAN, 2 EUROPEAN, 1 JAPANESE)
- . OPERATION IN 1983.
- . IDEAL EXAMPLE OF JOINT PROJECT.
- . POSSIBLE EXTENSION TO TORE SUPRA (CADARACHE).

MATERIALS (US, EUR, JAPAN ET AL.)

1. COOPERATIVE EXPERIMENTS WITH WIDE EXCHANGE OF RADIATION DAMAGE DATA : SATISFACTORY STARTING.
2. DESIGN AND CONSTRUCTION OF THE FMIT FACILITY (LOS ALAMOS→HANFORD)
   - . DIFFICULTIES DUE TO US BUDGETARY CONSTRAINTS
   - . EUROPEAN ENCOURAGEMENT RECOMMENDED BY BECKURTS PANEL
   - . POSSIBLE MODIFICATION OF THE AGREEMENT REQUIRING UNANIMOUS EC COUNCIL DECISION.

ALMOST READY FOR SIGNATURE : REVERSED FIELD PINCHES
US, EUR, JAPAN

1. COORDINATION OF ZT 40 (LOS ALAMOS), ETA BETA II (PADUA), HBTX 1A (CULHAM)

2. CONSTRUCTION AND OPERATION OF RFX, JOINTLY DESIGNED BY LOS ALAMOS, PADUA, CULHAM.

   - . INITIALLY PLANNED AT CULHAM, CANCELLED BY UKAEA
   - . US PARTICIPATION IN HARDWARE AT 8 M DOLLARS LEVEL INITIALLY ENVISAGED
   - . PROPOSAL BY PADUA UNDER REVIEW
   - . US POSITION NOT YET DEFINED.

SLIDE 17

Let me go now to the International Energy Agency, which Dr. Mori already mentioned (slide 17). TEXTOR is going very well. We have partial joint construction and intention for joint operation by Europeans, Americans, Japanese and also some others. Legal commitment has been excellent. I heard in Washington and Baltimore that new important collaborative actions on this machine are in discussion. Dr. Mori clearly spoke on the matter of superconducting coils. This seems to me an ideal example, because this facility was six coils, and you, the United States, provide three coils plus facility, which is important, and there are two European coils and one Japanese coil, which is already the first completed. So with a limited cost from each side, all the members have the full benefit of a full toroidal set of superconducting coil magnets.

With regard to materials, there are two elements already in place.

First, there are cooperative experiments with wide exchange of radiation damage data.

Second, there is the proposed construction of the FMIT facility by the United States with some staff participation by Europe and Japan. We know the financial difficulties of the United States. These difficulties were already known while the Beckurts panel was working and encouraged the United States with going on with this approach.

We have considered some steps in order to prepare a possible participation of Europe and Japan to this construction. I have to say that this requires modification of the agreement, which requires unanimous decision by the EC Council. This action we are considering with a positive approach at this level of science.

Again, in the frame of the IEA there is almost ready for signature a fourth agreement on the reversed field pinches. In the beginning, as I said before, this machine was planned for construction at Culham. The United States participation in hardware for the Culham machine use was initially envisaged at the level of $8 million.

With regard to the Padua machine, we don't know what the decision will be in this case. We have no formal agreement as we have, for instance, with Japan and Soviet Union for cooperation in the future between EC and US; we have no formal agreement. Nevertheless, bilateral cooperation is going on on an informal basis, especially on this subject. Two years ago we had a meeting in Washington. We recognized at least at this moment that the two programs were complementary. We are concentrating on preparing for developing accelerators and proposals for concrete collaboration since the Beckurts panel recommended cooperation.

BILATERAL US-EURATOM

NO FORMAL SPECIFIC AGREEMENT BUT SOME COOPERATION IN PROGRESS.

ALTERNATIVE LINES : DURING JOINT REVIEW (WASHINGTON, MAY 1980) THE TWO PROGRAMMES WERE FOUND COMPLEMENTARY

EUR : STELLARATORS, RFP'S
US : MIRRORS, BUMPY TORUS, COMPACT TOROIDS ETC.

JOINT COMMITTEE CONSTITUTED FOR STELLARATORS : PROPOSAL FOR CONCRETE COLLABORATION EXPECTED SHORTLY.

CONTACT OFFICERS APPOINTED FOR OTHER LINES.

BECKURST PANEL RECOMMENDED ACTIVE EUR PARTICIPATION IN US MIRROR PROGRAMME. NO INITIATIVE UNTIL NOW.

TOKAMAK : THE TWO PROGRAMMES ARE LARGELY OVERLAPPING.

COLLABORATION IN PROGRESS : . REGULAR MEETINGS TFTR - JET
. ORGANISED COOPERATION PRINCETON-FONTENAY.

ON SPECIALIZED TOKAMAKS :

. HIGH FIELD IGNITION DEVICES : COOPERATION ORIGINALLY PLANNED ON ZEPHYR (GARCHING) - PROJECT CANCELLED DUE TO HIGH COST - NEW, CHEAPER PROPOSAL UNDER REVIEW - IF FOUND FEASIBLE, OPEN FOR COOPERATION.

. TORE SUPRA

SLIDE 18

In the Tokamak, the two programs are largely overlapping (slide 18). For instance, with the two machines, TFTR and JET, there are regular meetings, by the directorates of the machines. Some co-operation was envisioned for a new Tokamak but unfortunately this machine has been cancelled. Active cooperation on Tore Supra is under consideration.

HEATING : BETTER COORDINATION AND SOME TASK SHARING DESIRABLE.

NEXT STEP AND RELATED TECHNOLOGY

INTOR SOLUTION UNCERTAIN

IS IT POSSIBLE AND DESIRABLE TO START A TIGHT COOPERATION ON THE NEXT STEP(S) AND RELATED TECHNOLOGY, ON A BILATERAL OR TRILATERAL (JAPAN) BASIS ?

MINIMUM AIM : FED/NET COORDINATION
MAXIMUM : JOINT DESIGN IN VIEW OF JOINT CONSTRUCTION.

CONCLUSION

SEVERAL COOPERATION ACTIONS IN PROGRESS
SEVERAL NEW ACTIONS DESIRABLE.

AN "UMBRELLA AGREEMENT" ON FUSION
- WOULD NOT BE EMPTY
- WOULD ALLOW BETTER COORDINATION AND BALANCE OF CROSS-PARTICIPATION.

CENTRAL POINT : A CONCERTED OR COMMON STRATEGY ON THE NEXT STEP WOULD HIGHLY IMPROVE EFFICIENCY.

SLIDE 19

Let met finish with this. The INTOR solution was ideal, but as I say, the probability of completion is rather small. I am speaking for myself, not officially, and for a certain number of my colleagues; I think it is possible to start a cooperation on the next step, related technology on the basis of bilateral or even better, trilateral, that means with the Japanese (slide 19).

The minimum aim should be regular coordination between the programs. Let me say NET is our next step, I now know the name has changed, but this should be minimum. The maximum should be to do it in view of a joint effort. I know this is perhaps too optimistic, but I think we should have a look on the possibility of this.

In conclusion several cooperative actions are possible. If we could reach an agreement concerning fusion between you and us, it would allow better coordination and balance of cross participation. I think the essential point should be on the next step.

Thank you Mr. Richardson.

Thank you very much, Dr. Palumbo. I have to pause a moment and take stock of our schedule. I was just told that you, Dr. Mori, and Dr. Clarke have a meeting at 4 o'clock. So we will want to finish our discussion before then.

In addition, we want to hear from Steve Rockwood and Richard Schriever. May I ask, Steve, how much time you will require for your presentation?

Dr. ROCKWOOD. I had planned on 15 minutes.

Mr. RICHARDSON. That is quite comfortable.

Richard.

Dr. SCHRIEVER. Ten.

Mr. RICHARDSON. Then I believe we will be able to keep well to our schedule.

It is wonderful to have such a cooperative spirit. All right, we are in good shape. I propose then that we turn to this extremely important question Dr. Palumbo has raised of the extent and benefits of international cooperation, and focus on that for the next few minutes.

Questions?

Dr. WATSON. I had the impression Dr. Palumbo said that the Tokamak programs of the United States and Europe were largely overlapping. I also had the impression from presentations given this morning that they were largely complementary. I was wondering if someone could address this. There seems to be a difference of opinion.

Do you want to address this, John?

Dr. CLARKE. Could you repeat the question?

Dr. WATSON. Dr. Palumbo expressed the opinion that the Tokamak programs of the European and U.S. communities were largely overlapping. I though this morning we said the programs were largely complementary. We seem to have two different opinions on that.

Dr. CLARKE. I think the explanation is in the adjective "largely" As Dr. Palumbo indicated, Europe is devoting the majority of its efforts to Tokamaks. Since we are devoting a large effort to Tokamaks we are totally overlapping in that area. But there is more to our program than that. We have a lot of technology activities. We have the majority of effort in the magnetic mirror program. So in the magnetic mirror program we are complementary to what is going on in Europe and also what is going on in Japan. I think if you look at the whole picture of what is going on in the United States, you would conclude that it is complementary in many aspects with what is going on in the rest of the world.

But it is by no means exclusive. You are going to find some overlap.

Dr. GOTTLIEB. It is inevitable that if you are building three Tokamaks they are going to overlap because they still have the same fundamental mode of operation. But the emphasis in each of the programs is different. The U.S. Tokamak, TFTR, is you might say the simplest construction. It should be, one hopes the easiest to modify. It is smaller and therefore easier to heat. We made it the minimum size deliberately to accomplish the objectives we set out to accomplish, mainly energy balance.

The European approach, the Joint European Tokamak (JET), was somewhat different, a more ambitious approach, larger than the Princeton Tokamak and closer to reactor size, involving not a circular but an elongated plasma. The two will have a different set of properties and it will be useful to check one against the other.

Now both of them may be used with deuterium and tritium so they may be used with actual fuels to demonstrate power output and in addition, to get some experience with the technical problems or engineering problems involved in handling these fuels. The Japanese approach is different from both. They felt that it is very important to understand as thoroughly as possible the basic physical ideas taking place. They chose not to contaminate the system with tritium, but to utilize hydrogen. But they chose a conservative design featuring a divertor which is a technologically difficult task, and that complicates the construction rather considerably. But this is a feature which Tokamaks may well require. We are not absolutely sure. Neither the TFTR nor the JET have this feature.

Each group was able to see what the other groups were doing and wanted to make them different to emphasize different aspects of the total problem. The Russian approach is different still, in that they are going to emphasize very long pulses and use electron cyclotron heating which is different from the others, and use superconducting coils.

Now TFTR is the simplest, it will be in operation first. JET is close behind, perhaps 6 months behind, a more complicated one. The JT-60 has additional complications. It is perhaps 2 years behind, Dr. Mori?

Dr. MORI. Yes.

Dr. GOTTLIEB. The Russian device is about 3½ years behind. Different levels of complication are involved. They will supplement each other beautifully.

Dr. PALUMBO. Can I respond?

First the words should not be taken out of context.

As I said before we recognize that among alternative lines we would complement the United States. That means that the United States may have 30, 40 percent of their effort on things which for us it is zero. We devote to stellarators a small part of our budget and your effort is nearly zero.

So in this sense on the alternative lines, we are on different levels. On the Tokamak both the United States and Europe have large Tokamak programs. Each American machine without any exception is different, I think, from the European machines.

There are no identical machines. But, of course, the aim of the two programs are identical: to improve performance, and utilize the Tokamak to develop the plasma programs and develop the panoply of the areas.

What I would do is draw this conclusion along alternative lines, if we were to establish cooperation, it could be possible that one party take responsibility for one line, and the other party for a different line. This should be impossible for the Tokamak. For Europe to take the Tokamak and United States to discontinue or vice versa, we are deeply involved in this and that cannot be done. In this sense, you should interpret my terms in that way. In this sense, it is true.

Mr. RICHARDSON. Other comments?

Dr. DEAN. You showed a graph of the relative size of the European and Japanese efforts relative to the U.S. effort. The bar chart. It is true that the United States figures include manpower costs and construction and the other figures do not include manpower?

Dr. PALUMBO. Sorry. There is a design difficulty in comparing it. In Europe we include capital cost except for the buildings, for the concrete. Not for building machines, but for building the buildings in which the machines are located. One laboratory for a Tokamak needs a new building, concrete building, to put this machine in. We contribute 25 to 45 percent costs of machine, but for the building we pay rent. So the capital cost for the concrete is not included, but it is the sizes are included, in Europe they are. Not in the Japan.

Dr. MORI. We have not authority to do that.

Dr. PALUMBO. In Europe, yes, we do.

Dr. GOTTLIEB. I just wanted to mention that there are some tensions between on the one hand the industrial cooperation and on the other hand international cooperation. I am not saying, therefore, one should not have both of these. One should. But they do create tensions. As an example, if one were to build an INTOR or something like it with bilateral, trilateral or any number of participants, industry would want to participate, of course, in each country. But in each country each industry would have the feeling, "we cannot afford to let the other nation know all about some aspect of the program and get the know-how while we are left out."

We already have seen things like this emerge. For example, if one country took care of the tritium plans, the other nations would be very much concerned that while they will get all the drawings and specifications they really won't get the know-how to handle tritium. It is one aspect of the situation. I was going to ask Dr. Mori to comment on this, but I will not because he has to get away.

Dr. MORI. Well, let me say, the Japanese industry is paying a lot of money for fusion technology. One example is the JT-60. It is welded by a new welding technology, technology developed for the purpose of the JT-60, and it is now used for other heavy metal manufacturing.

Mr. RICHARDSON. I can see this is a delicate problem of allocating the benefits. That adds to your difficulties.

Thank you, Dr. Mori.

Mr. GAVIN. It is worse than that, if I may say. While the collaboration of the next step may be desirable and the parties agree, there is a point in the future somewhere where one could expect to encounter the same difficulties that exist currently in the aircraft industry and in the steel industry, and these difficulties revolve around the difficulties that exist in different nations in exporting centers, tax structures and favorable government financings.

This really, I think, is something that should be considered at this point even though the impact may be years and years in the future.

Certainly, it does have a bearing on industrial participation.

Mr. RICHARDSON. Where are these issues being considered, Joe. Are they being considered in the administration, in the Congress?

Mr. GAVIN. Some are not being considered adequately, but I believe the GATT negotiations are basically the place.

Mr. RICHARDSON. Does GATT take account of fusion?

Mr. GAVIN. Anything that involves exports at a later date is unusually in GATT.

Dr. GOTTLIEB. I would just say that it isn't so simple: That to achieve international cooperation, all you have to do is get four people in a room and have them sign something.

Mr. RICHARDSON. I think that is clear, yes.

I would defer to Jack Dugan as to how how far to pursue the ins and outs of this.

Mr. EVANS. I would second what Dr. Gottlieb says. I recently poked a button at the computer and it spit out references on exchange of technology and regulations and so on. It was overwhelming. I got a small stack of reprints from the service and I couldn't believe the multifaceted thing, within one Department of the Government you find intense convictions 180 degrees from each other of what is important and what is not, what can be exchanged and what cannot be. It is mindboggling. We have just not faced that. We would get awfully tangled up if we do.

Mr. RICHARDSON. Since our principal speakers have left, this is a natural time to terminate this part of the workshop. I would suggest we need a break of about 10 minutes. Then if we could reconvene at 5 after 4, this will give us time to hear Steve Rockwood and Dick Schriever comfortably and somewhere from minus 2 minutes to 30 minutes from Mel Gottlieb.

[Recess.]

Mr. RICHARDSON. OK, if we can get back to work again.

Are you going to lead off, Dick?

STATEMENT OF DR. RICHARD L. SCHRIEVER, DIRECTOR, OFFICE OF INTERTIAL FUSION, U.S. DEPARTMENT OF ENERGY

Dr. SCHRIEVER. Yes I will (Slide 1).

INERTIAL FUSION PROGRAM

BRIEFING

by

Dr. Richard L. Schriever
Director, Office of Inertial Fusion
U. S. Department of Energy

for

FUSION ENERGY WORKSHOP

Sponsored by

The Senate Energy and Natural Resources Committee's Subcommittee on Energy Research and Development and the House Committee on Science and Technology's Subcommittee on Energy Research and Production

September 8, 1982

SLIDE 1

Mr. RICHARDSON. My plan is to give you about 15 minutes or so and give Steve about 15 minutes. That will take us roughly to 20 minutes to 5 or quarter to 5. Mel will have 15 to 20 minutes to conclude. We will adjust that if it is not satisfactory.

Dr. SCHRIEVER. That is fine. We will be brief with the hope that anyone who has not heard all they wanted to about fusion already will have a little interest left for Dr. Gottleib.

What I would like to do this afternoon, rather than go into any details or discussion of the inertial fusion program, is to try to leave us all with a perspective of how the program as it is now in place relates to fusion energy, basically that perspective of the future of inertial fusion (slide 2).

DEPARTMENT OF ENERGY
INERTIAL FUSION PROGRAM

OBJECTIVES

R&D TO DETERMINE FEASIBILITY OF RELEASE OF SIGNIFICANT THERMONUCLEAR ENERGY FROM SMALL FUEL PELLETS HEATED BY LASERS OR PARTICLE BEAMS

- **USE THIS ENERGY FOR WEAPON PHYSICS STUDIES, WEAPON EFFECTS SIMULATION**
- **DETERMINE ITS POTENTIAL USE AS AN ENERGY SOURCE IF HIGH GAINS PROVE TO BE FEASIBLE**

SLIDE 2

Inertial fusion is very much a research and development activity at this point. We are interested in achieving an understanding of how one would ignite a small amount of thermonuclear fuel by heating it with a laser or particle beam. This program is organizationally separate from the Magnetic Fusion Energy program. As Director of the program, I am responsible to the Assistant Secretary for Defense Programs. Like all nuclear programs, including the magnetic fusion program, all of these programs began in, or are derivatives from, the weapons program.

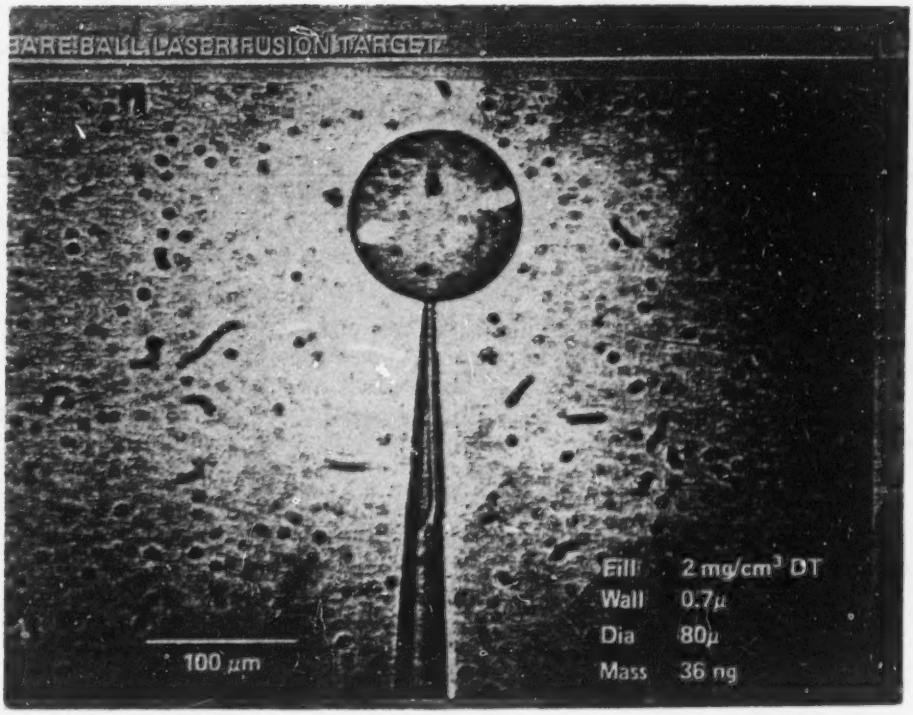
The inertial fusion program in fact still is in that context. This comes about because much of the work is classified. A small but significant part is, however, unclassified.

Much of the work is done in the nuclear weapons laboratories, and the program benefits very much from the capabilities and background that exists there.

Inertial fusion has two applications which can result from a successful research program in this area. The first of these is the application of the physics lessons learned to the technology of the nuclear weapons. Aside from nuclear weapons, inertial fusion is one of the few areas where laboratory research can be conducted in very, very high temperature and density states of matter.

As a result, this program is of immediate interest and near-term benefit to the weapons laboratories and to the weapons program. If we are successful in the activities that we have undertaken in learning to understand how to ignite a small thermonuclear pellet, there will be a payoff in terms of energy applications of inertial fusion. Those lie at some time in the future.

One of the objectives of our program is to develop a basis of understanding that would allow us to make a decision to proceed with an engineering development program for energy.



SLIDE 3

Slide 3 illustrates a small inertial fusion pellet. As you see, it is a few hundred microns in diameter. It is a simple glass micro balloon. The reason for putting that up is to illustrate that some of the engineering problems and solutions are uniquely different from those we have been discussing today even though the fusion fundamental process is exactly the same.

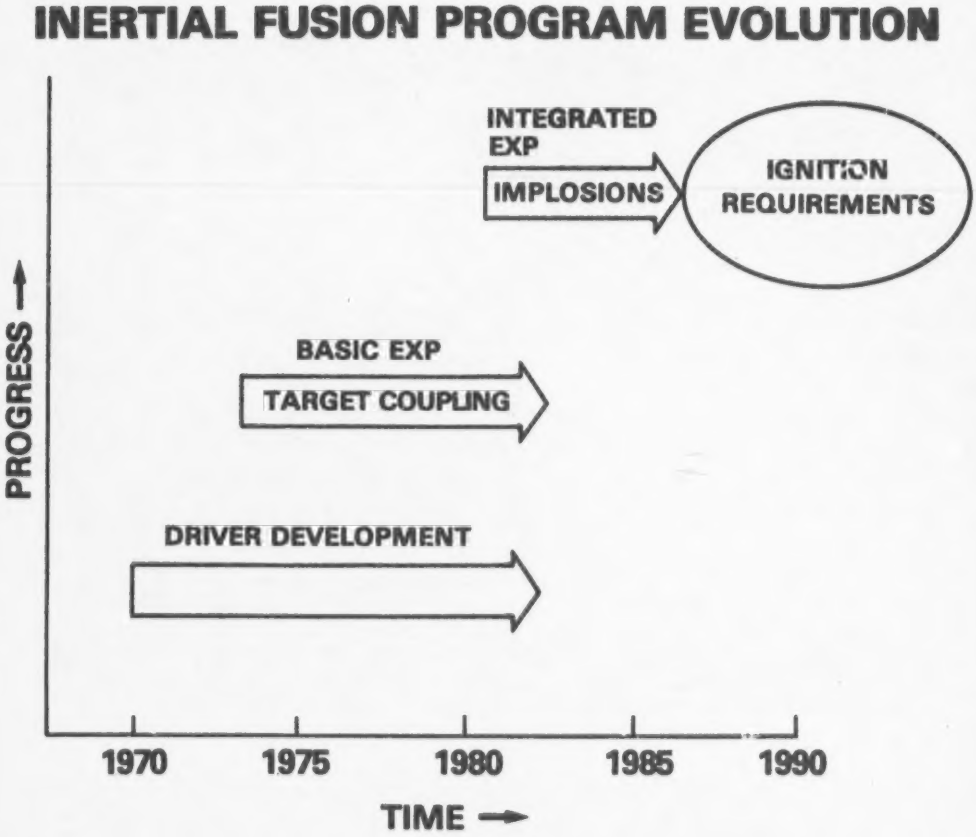
This serves to illustrate somewhat the nature of our program; even though our reactor is very, very tiny there, the resources that we have had to apply to do those experiments are very significant.

Mr. RICHARDSON What is inside the balloon?

Dr. SCHRIEVER. That balloon contains probably 100 atmospheres of deuterium-tritium fuel. The surface of that would be irradiated somewhat uniformly by an array of laser beams, heated very rapidly, caused to compress, brought to a very high temperature and density, and undergo thermonuclear ignition.

Mr. RICHARDSON. That is 100 atmospheres at room temperature?

Dr. SCHRIEVER. Yes, and that depends on the particular pellet we are interested in.



SLIDE 4

In order to approach the study of that very small pellet, it has been necessary to develop very large experimental laser and particle beam facilities. (slide 4) We will briefly look at those in just a moment.

A great effort has gone forward in our program over the last ten years to develop those so-called driver facilities—the lasers and particle beams we are interested in. It involved a great deal of expense and has been a preoccupation with most of our activities. In the last few years we have reached a point where we could do basic target experiments looking at the coupling of laser light and particle beams to these tiny pellets. We are reaching a stage now in the early 1980s when we have facilities coming on to line that are large enough. We are in a position now to do what we call integrated implosion experiments, wherein we look at the overall behavior of an irradiated pellet and try to understand what the requirements would be to implode it successfully and bring it to ignition. We expect in this program to begin to have a more complete understanding of those ignition requirements in the late 1980s.

I might take a moment to point out some basic differences between the inertial fusion and magnetic fusion.

In order to do these experiments, very few of the engineering problems have to be solved in advance. We have to have the laser,

which has relatively simple requirements compared to what would be needed for a power reactor. We basically then direct that into a target and do physics on it and perhaps at some point successfully ignite it.

At that point in time you could then separately begin to approach the problems of engineering development. I would contrast that to something like a Tokamak where, before you succeed with the experiment, you must to a large degree solve a large number of basic engineering problems.

In TFTR, when it comes on line, many of the problems will have been addressed and partially solved. So there is a difference in the way that the programs would approach an energy application. As a result, looking at the level of resources in this program and as it goes on, we focus more and more on the basic target interaction experiments and at the present time devote no major activities in the program to engineering development for power applications. The notable exceptions to this are some activities that are sponsored by EPRI. We have heard about some of that today. Also there are a number of system-type studies that have been done at the discretionary level within the laboratories.

DEPARTMENT OF ENERGY
INERTIAL FUSION PROGRAM

INERTIAL FUSION PROGRAM STATUS

- **RECENT EXPERIMENTS DEMONSTRATE FUSION BURN AND HIGH COMPRESSION**
- **EXPERIMENTAL FACILITIES NOW BEING BUILT WILL ADDRESS ENERGY REQUIREMENTS FOR PELLET IGNITION**
- **EXPERIMENTS AND THEORY INDICATE ENERGY APPLICATION WILL REQUIRE DRIVER ENERGY TEN TIMES LARGER THAN CURRENT FACILITIES**

SLIDE 5

At the present time we have completed a series of experiments (slide 5) which begin to tell us the basic compression requirements, achieving high densities and beginning to understand symmetry requirements, some of the difficulties of pellet fabrication, and we are ready to move into another regime of physics.

We are now completing major experimental facilities which will address these energy requirements and give us an opportunity to complete our understanding of the subject. I think the basic message that we have learned from these experiments is that the driver energy requirements to achieve an energy yield sufficient to

be useful in a power application are now much larger than originally anticipated and are probably on the other of ten times as large as the facilities we are now bringing on line. So there is a significant additional step to be made after the current set of experiments.

DEPARTMENT OF ENERGY
INERTIAL FUSION PROGRAM

PROGRAM PARTICIPANTS

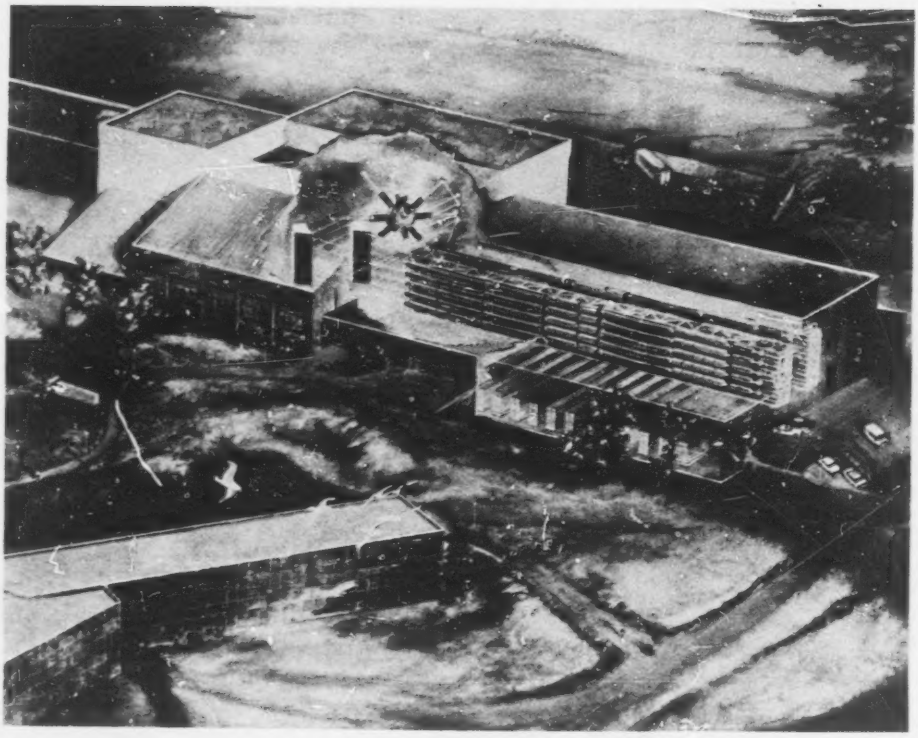
LAWRENCE LIVERMORE NATIONAL LABORATORY LIVERMORE, CALIFORNIA	GLASS LASER EXPERIMENTS
SUPPORTING LABORATORIES:	KMS FUSION, INC, ANN ARBOR, MICHIGAN UNIVERSITY OF ROCHESTER, NEW YORK NAVAL RESEARCH LABORATORY, WASHINGTON, DC
LOS ALAMOS NATIONAL LABORATORY LOS ALAMOS, NEW MEXICO	CO_2 LASER EXPERIMENTS
SANDIA NATIONAL LABORATORIES ALBUQUERQUE, NEW MEXICO	PULSE POWER (ION BEAM) EXPERIMENTS
SUPPORTING LABORATORY:	NAVAL RESEARCH LABORATORY, WASHINGTON, DC

SLIDE 6

Participating in this program at the present time (slide 6) are the National Laboratories and three principal support laboratories, KMS fusion in Ann Arbor, Michigan, the University of Rochester, sponsored by the Department of Energy and a consortium of industrial and university sponsors; and also the Naval Research Labortories in Washington.

The Lawrence Livermore laboratory is the lead laboratory for glass laser or short wavelength experiments and they are one of our major experimental programs with laser facilities.

Los Alamos has responsibility for long wavelength lasers, primarily carbon dioxide lasers. That is the second important element in our program.

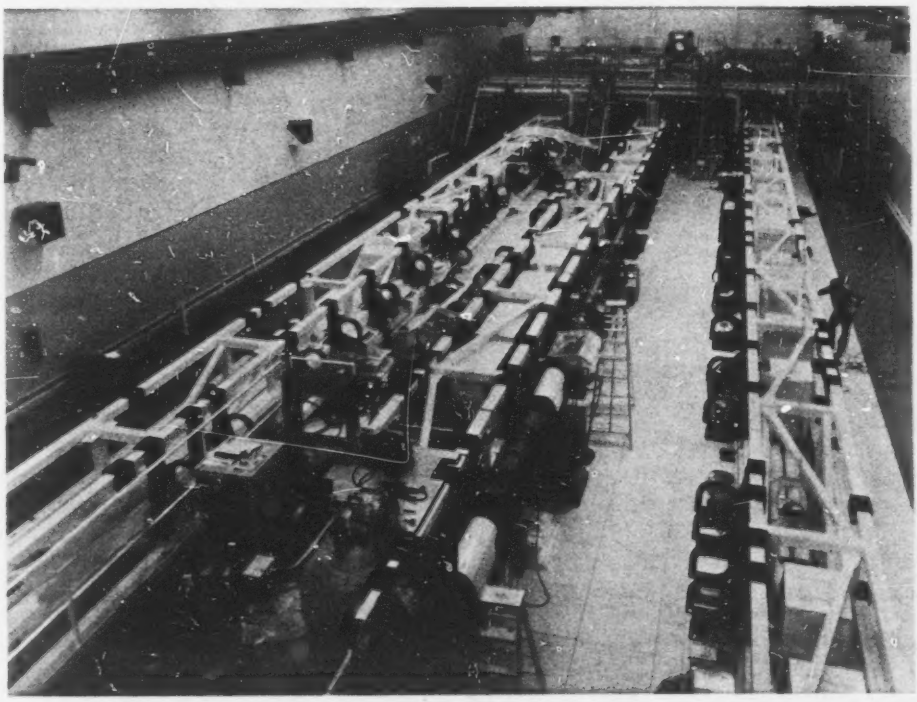


SLIDE 7

The third major activity is under Sandia Laboratories, which conducts pulsed power, or ion beam, experiments. (slide 7) This is an attractive, low cost option for driving fusion pellets that we are seriously interested in at the present time.

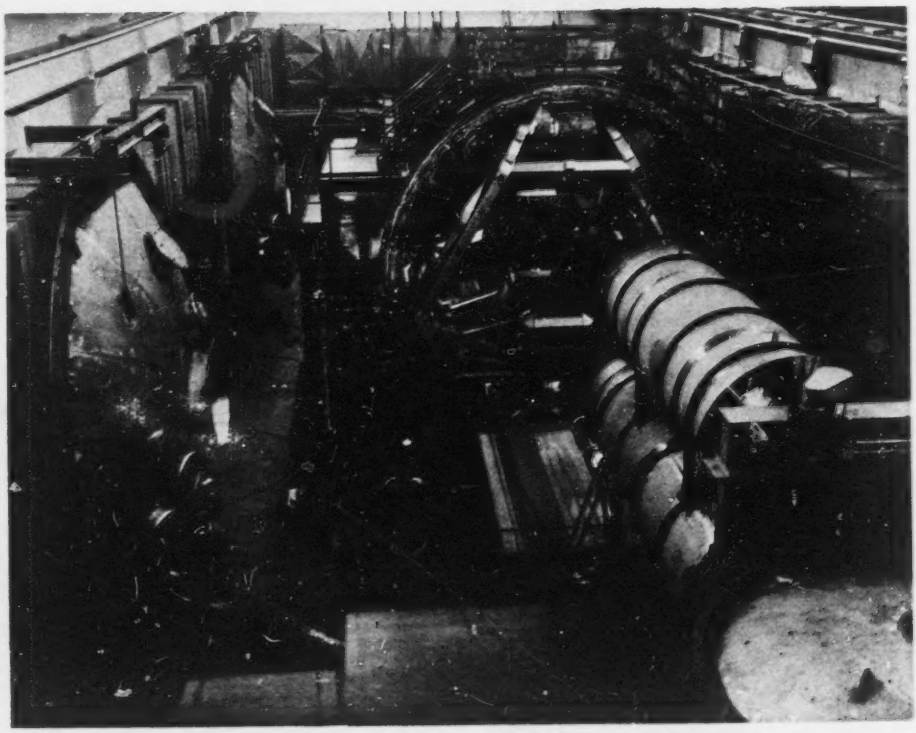
Briefly, to review these major facilities, the first of these is to be the NOVA laser at Livermore. It is approximately an order of magnitude upgrade over the previous system. It will have ten beams, 100 kilojoules of basic energy power to deliver to the pellet in the

target chamber. A significant feature of this experimental facility is that it will be wavelength converted. That is, it will be designed to operate both in the infrared at green wavelengths and at blue or the near ultraviolet wavelength. This is a very important development because as we have reached this point we found that for this kind of target experiment a shorter wavelength is advantageous.



SLIDE 8

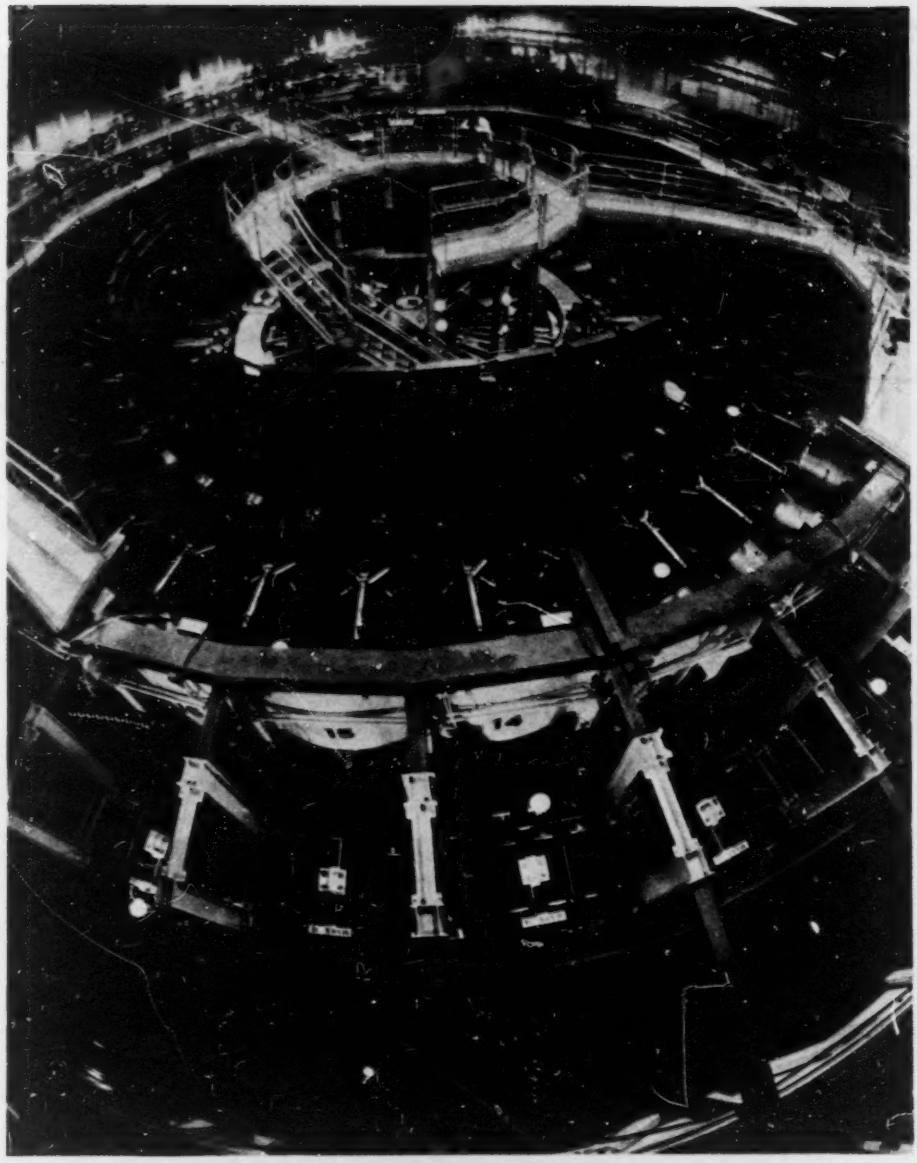
That facility will be complete towards the end of 1984. In Slide 8 you see the first two beams of NOVA. They are being installed in a facility called NOVETTE. They will operate for the next 2 years and then be incorporated into the full system. This experimental facility will be completed in November and will give us a step forward in the short wavelength experiments into an area of power intensity that we have not been able to arrive at before. This is a major near-term step in the program.



SLIDE 9

The next major facility shown in the target chamber arrangement of the Antares CO_2 laser at Los Alamos. (Slide 9). The people in the lower part of the picture gives you a size of the immense scale.

This laser develops about 40 kilojoules of 10 micron CO_2 laser light. It will be completed in 1983 and give us an opportunity to fully evaluate a number of very exciting target concepts which are now being developed on the smaller Helios laser.



SLIDE 10

The final of the major three facilities in the pulse power machine, PBFA–1 and 2. (Slide 10). This is PBFA–1, the first of the two machines. It is a large device, 60 or 90 feet in diameter, with stored energy on the outside delivering pulsed power to an ion source at the middle. The major light ion experiments will be conducted and understanding will be developed on this machine.

DEPARTMENT OF ENERGY
INERTIAL FUSION PROGRAM

PROGRAM PLAN

- DIRECTED TOWARD WEAPONS PHYSICS EXPERIMENTS
- EMPHASIZE COMPLETION OF THREE MAJOR EXPERIMENTAL FACILITIES
  - ANTARES
  - NOVA
  - PARTICLE BEAM FUSION ACCELERATOR II
- SEEK A MORE COMPLETE UNDERSTANDING OF PELLET INTERACTION PHYSICS THROUGH EXPERIMENTS USING THESE FACILITIES
- LATE 1980's ASSESSMENT OF ENERGY POTENTIAL AND FUTURE PROGRAM DIRECTIONS FOR INERTIAL FUSION

SLIDE 11

To summarize, then, looking at the program plan (slide 11) that is built around these facilities, primarily our activities are directed at the present towards weapons physics experiments, and the many things that we become aware of as we study the basic thermonuclear inertial fusion process.

The program will emphasize the completion of these three major facilities that we have discussed and we will conduct the program in a way that will allow us to obtain the most information possible and gain the most complete understanding from those facilities. We will do all we possibly can within the current planning and budget constraints that we have heard so much about today.

On the basis of these experiments and the understanding we hope to develop, it is our intention to make a major program decision in the late 1980's. We have done our planning to establish the decisions and criteria, and the basis for reaching those decisions, and that will determine future directions for the inertial fusion program.

Those decision options could range from deciding to go no further with the program, to developing a large single facility of the most appropriate design to continue the work, or possibly even entering at that point into major engineering development for energy applications.

I think, though brief, that gives you some orientation as to how the program is laid out and how it will eventually come to apply itself to the energy applications that we are here to talk about today.

Thank you.

Mr. RICHARDSON. Thank you, Dick.

I would suggest we hold questions until after we hear from Steve Rockwood, unless there is a point of clarification someone would like to ask.

STATEMENT OF DR. STEVE ROCKWOOD, LOS ALAMOS NATIONAL LABORATORY

Dr. ROCKWOOD. This afternoon I will principally talk about a small portion of the ICF effort Los Alamos has been responsible for administering for the Department of Energy as a lead laboratory, and specifically the heavy ion approach to fusion. I will not discuss our work on the CO_2 laser which is the principal part of our program supported by defense programs. I choose to do this at this time because the heavy ion effort has been supported by the Assistant Secretary for Defense programs for the last several years but is now being proposed for support by the Office of Energy Research.

Of all the drivers that have been envisioned for inertial confinement fusion, the accelerator is one that a person can most easily conceptualize as being used in civilian missions. So I feel the accelerator approach may be of the highest interest for the audience today. Also, a memorandum of understanding was signed last spring between the Assistant Secretary for Defense programs and the director of the Office of Energy Research. I believe Dr. Leiss, in his testimony to House Science and Technology Committees this spring, referred to this planned transfer of administration and direction of the development of heavy ion accelerators as inertial fusion drivers from defense programs to OER. For those two reasons I will dwell principally on the heavy ion accelerator.

Let me start by presenting what one might call a road map for inertial confinement fusion similar to those that are presented for magnetic confinement fusion. You have a principal criteria, that is that the product of fuel density, rho, and radius, in which density is confined, reaches a certain value that is on the order of 1 gram per square centimeter. If one looks at the simple scaling of that product, rho-r, being constant, you find the density to which you have to confine the fuel increases with the reciprocal of the square root of the mass of the fuel you are going to use.

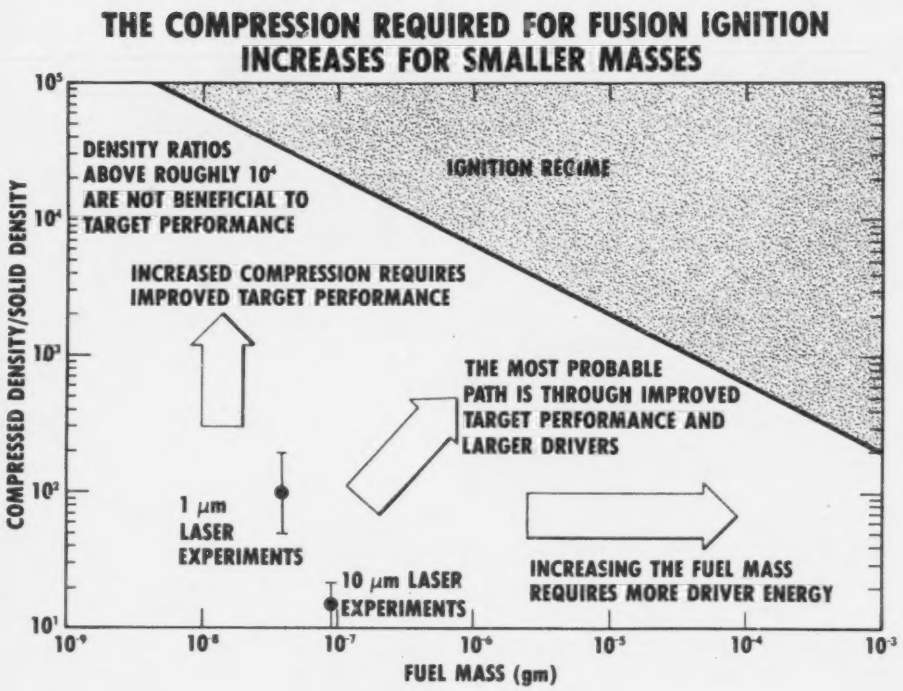

THE COMPRESSION REQUIRED FOR FUSION IGNITION INCREASES FOR SMALLER MASSES
DENSITY RATIOS ABOVE ROUGHLY 10⁴ ARE NOT BENEFICIAL TO TARGET PERFORMANCE
IGNITION REGIME
INCREASED COMPRESSION REQUIRES IMPROVED TARGET PERFORMANCE
THE MOST PROBABLE PATH IS THROUGH IMPROVED TARGET PERFORMANCE AND LARGER DRIVERS
1 μm LASER EXPERIMENTS
10 μm LASER EXPERIMENTS
INCREASING THE FUEL MASS REQUIRES MORE DRIVER ENERGY
COMPRESSED DENSITY/SOLID DENSITY
FUEL MASS (gm)

Figure 1

Bob Scott made the point this morning, and I believe it is a very important one not to lose sight of, that of the approaches to fusion man has tried, inertial confinement is the only one we have made work to date, albeit on a very large scale. Now, as we start decreasing the fuel mass, this reciprocal relationship forces the density to which one must compress the fuel to increase, and because of this scaling problem, life becomes much tougher for fusion on a small scale. The smaller the pellet, the smaller the mass, the better it must be compressed and the better it must be manufactured (Fig. 1).

I have shown two data points, one obtained with a 1 micron laser, the other with 10 micron lasers, which represent the status of inertial confinement today. If you are to achieve no better compression than that presently shown, about a hundred times solid density, in itself no mean feat, then you can see the mass must be scaled substantially to reach the ignition regime of rho-r approximately equal to 1. Scaling up the mass of the target implies scaling of the energy of the driver because of the heat capacity.

That is a scaling of many orders of magnitude. Preferably, one would like to stay with the energy of the present drivers and just increase the compression. But that drives you into densities that are such that further compression is really not beneficial. The most probable path indicated by the diagonal line is that one must get better compression, on the order of 1,000 times solid density, and, at the same time, increase the energy of the drivers. The data points shown are obtained with roughly 10 kilojoules of driver energy. Thus, very naively, from this plot one can see the need for

a driver with an energy on the order of a megajoule to reach the ignition regime. That, within a factor of a few, is supported by present calculations.

INERTIAL FUSION IS AN IMPORTANT ENERGY OPTION

- IT IS KNOWN THAT ICF WORKS
- ICF ENGINEERING PROBLEMS ARE DIFFERENT THAN MFE ENGINEERING PROBLEMS
- THIS DIVERSITY GIVES FUSION A BROAD BASE AND INCREASES THE PROBABILITY OF SUCCESS.
- SOME OF THE ATTRACTIVE FEATURES OF ICF, DISCUSSED BY THE DOE AD HOC EXPERTS GROUP ON FUSION, ARE LISTED ON THE NEXT TRANSPARENCY.

Figure 2

If I could have the next slide (Fig. 2) I would like to iterate what the important points of ICF as an energy option are. The first we have already mentioned. The second Rick Schriever alluded to, namely it offers different avenues as solutions to engineering problems. The high technology elements of the program are, by and large, removed from the region of high radiation dosage. The laser or heavy ion accelerator, is some distance away from the reaction chamber where the pellet is being exploded.

UTILITY OF ICF

"One of the chief advantages of ICF is that...the driver is separate from the reactor vessel itself, and can be removed some distance. This means that an ICF reactor may have a relatively small containment volume; that its operation, maintenance and repair will be relatively simple; and that the most expensive components will not be subject to neutron bombardment and activation. The potential also exists for a great variety of power plant sizes and configurations."[1]

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[1]John S. Foster, Jr., AAAS Annual Meeting, 1980.

Figure 3

The next slide (Fig. 3) gives a quote from the Foster panel. This panel reviewed ICF about two years ago. I won't try to read it for you at this time. Rather, note that it points out what I was saying about the advantage to engineering of removing the high technology from the reactor chamber. I would make another point here. That is that once one has built an ICF reactor, improvement of that reactor's performance can almost be a trivial task if it can be accomplished by simply changing design of the small pellet. For example, suppose I have a pellet design that produces for me a gain of some factor. Then, in my research and development program I discover another design of a different capsule that increase the gain by a factor of three. Then the performance of my reactor is in fact up by a factor of three without having changed any of the expensive ancillary equipment such as lasers, reactors, or drive chambers.

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INVESTIGATION OF HEAVY ION FUSION DRIVERS IS MOTIVATED BY

- FAVORABLE TARGET COUPLING
- LOW BEAM CURRENT - BETTER FOCUSING
- HIGH DRIVER EFFICIENCY
- FAVORABLE COST SCALING WITH DRIVER SIZE

THE MAJOR PROGRAM GOAL AT THIS TIME IS TO DEMONSTRATE

GOOD BEAM QUALITY IN VERY HIGH CURRENT DENSITY ACCELERATORS

Figure 4

Let me now speak specifically to the heavy ion accelerator as the driver (Fig. 4). Ions, as opposed to coherent radiation, are, at least in theory, more favorably coupled to the target. Radiation, which is an oscillating electric field, is absorbed by electrons—the most mobile charged particles. In this process you give a lot of energy to some electrons, and they become so hot they preheat the fuel and make compression difficult. That is not believed to happen with particles impacting matter where the coupling obeys a classic law. Heavy ions are favored over light ions because heavy ions have a lower beam current. Thus, the focusing problems are greatly simplified.

Finally, for an energy application the efficiency of accelerators is presently considered to be greater than laser systems. That is certainly an advantage in going forward with any commercial application.

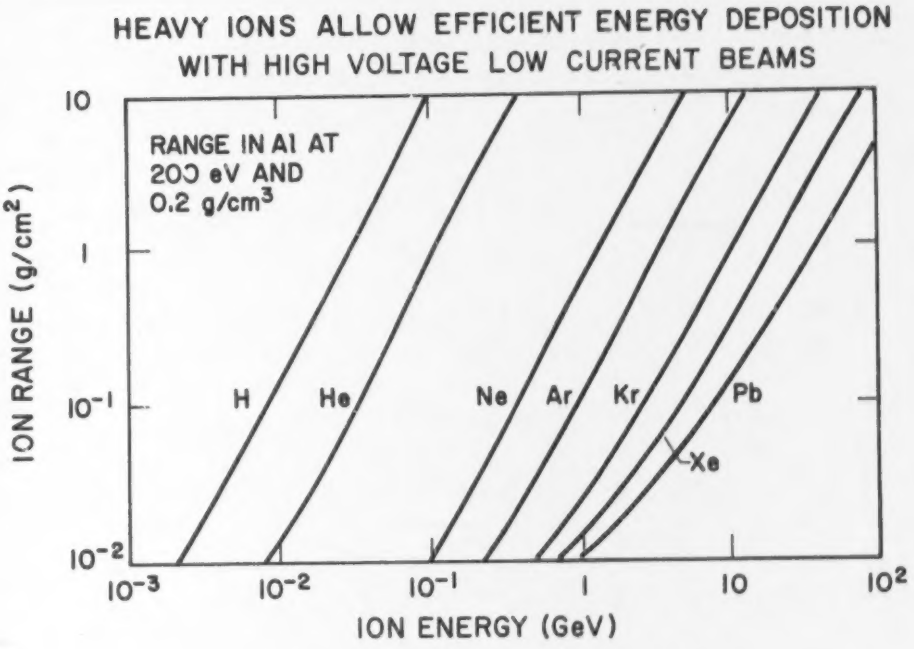

Figure 5

The fourth point that is noted is that accelerators are more economical to build than lasers, at least on a large scale (Fig. 5). Let me illustrate that second point of low beam current in one technical slide which I believe comes next. Let us say I want to deposit my energy or power of 300 terrawatts in a certain range of a tenth of a gram per square centimeter of material. Because the range of a heavy ion is much less for a given voltage than that of a light ion, and because I want to maintain a constant power, that is, the product of current and voltage is constant, I can increase the voltage of a heavy ion beam while keeping the range constant, and I can decrease the current in turn. You will see that going from mass unity for protons to masses of a couple of hundred, such as lead or uranium ions, you have reduced the current requirements from tens of mega-amps to tens of kiloamps. Focusing or transporting that beam is believed to be considerably easier.

CONVENTIONAL ACCELERATOR TECHNOLOGY IS WELL MATCHED TO POWER PRODUCTION

- Accelerators are reliable. Cern PS operates more than 90% of scheduled time.
- Accelerators have a long lifetime. Large accelerators built in the '50's are still operating
- Accelerators have a high pulse repetition rate
- Accelerators can be efficient. 15-35%.
- Except for high peak power, all ICF requirements have been demonstrated at existing machines

Figure 6

Let me address the characteristics of accelerators and how they relate to what one would like in a commercial application (Fig. 6). First, if you ask yourself what in the country today runs at average powers on the order of megawatts for very long periods of time with high reliability, accelerators come to mind. Their reliability is very high. CERN is up more than 90 percent of the time. Other facilities have equal on-line times. Accelerators also have long lifetimes. They have very few moving parts, if you will.

Intrinsic in some designs is a high pulse repetition rate that one would need for commercial applications, and the efficiencies I have already mentioned are considerably higher then existing laser systems. Looking at the long history of accelerators for inertial confinement, all of the aspects required have already been demonstrated in one machine or another. So there is a reasonable basis for confidence on proceeding.

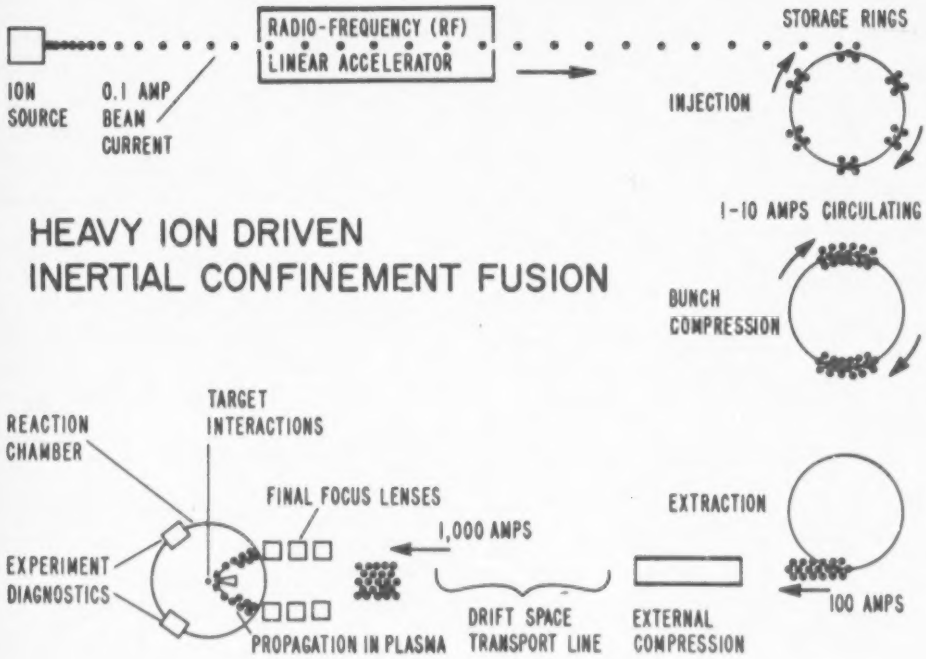

Figure 7

But don't be misled into thinking it is a trival task. We are talking of scaling up the current in accelerators by several orders of magnitude over what has been done to date. That is where the focus of the program rightly should be. The next Viewgraph (Fig. 7) shows very quickly how one would conceptually use an accelerator of the RF type. This program has been jointly pursued by Los Alamos and Lawrence Berkeley Laboratory. The particular design shown is an RF linear accelerator of the type developed at LAMPF. To build up the high peak power you take a conventional acceleration, and then you begin storing current in a storage ring, bunching the current, then extracting and focusing several such beams onto a target.

The main unknown in this approach is in the storage ring itself. Heavy ion fusion has not been a large program fundingwise, in fact trivial compared to anything else you have heard today. Thus, more work has been devoted to paper analysis than hardware. To get data quickly we have also looked at facilities around the world.

MAJOR PEER REVIEWS OF HEAVY ION FUSION HAVE BEEN FAVORABLE

- WORKSHOPS -	BERKELEY	1976 & 1979
	BROOKHAVEN	1977
	ARGONNE	1978
- JASON STUDY, JSR-77-41		1978
- AD HOC EXPERTS GROUP ON FUSION, 79-ETG-28 (SRD)		1979
- LOS ALAMOS STUDY, LA-8207-MS		1980
- EPRI STUDY, AP-1371		1980
- JOHN D. LAWSON, OXFORD SYMPOSIUM		1980
- LOS ALAMOS REVIEW BY LONGMIRE COMMITTEE		1981

Figure 8

Lawrence Berkeley Laboratory has proceeded along an induction line, which eliminates the need for the storage ring. Both the RF and induction approaches deserve futher consideration and certainly need further experimentation. As befits all new programs this one has had a proliferation, if you will, of reviews by peer committees. There were several workshops early in the formulation of the program (Fig. 8). Various Jason studies and ad hoc groups on fusion have reviewed it, and all with favorable results, I would say. In a technical sense no one has found the program to have a fatal flaw. The most recent review we have listed is the Jason panel under the direction of Burt Richter. They reviewed the program this summer. I haven't seen their report but have been told the panel was favorably impressed by the potential of this approach for civilian applications.

THE PROMISE OF ACCELERATOR FUSION RESTS ON 6 ASSUMPTIONS

1. ONE CAN BUILD AN EFFICIENT ACCELERATOR, AT REASONABLE COST, THAT CAN PUT MORE THAN 1 MJ OF ENERGY INTO A SMALL 6-D PHASE SPACE VOLUME.

2. THE BEAM CAN BE FOCUSED OVER A DISTANCE OF SEVERAL METERS ONTO A SMALL TARGET IN A REACTION CHAMBER.

3. PRESENT CALCULATIONS CORRECTLY DESCRIBE ION ENERGY DEPOSITION.

4. OUR SIMULATIONS DESCRIBE THE HYDRODYNAMIC AND THERMONUCLEAR BEHAVIOR OF TARGETS

5. TARGETS CAN BE CHEAPLY MASS PRODUCED.

6. AN ECONOMICAL TRITIUM BREEDING REACTOR CAN BE BUILT.

A REASONABLE PROGRAM MUST ADDRESS ALL ISSUES.

Figure 9

The next slide (Fig. 9) outlines the six major assumptions one makes in saying that heavy ion accelerators could be useful drivers for inertial confinement fusion, if the target physics works. These six assumptions should constitute the fundamental basis of a program, and the program should address each of these assumptions and prove or disprove it validity. This is what we have proposed for the heart of the program under OER. Beam transport and focusing are major issues.

The third point is challenged by some scientists. It may be tested in some later experiments. One must assume present calculations of target performance are in reasonable shape. This fact will be supported by the experimental programs on other drivers that Rick Schriever alluded to.

AN INTERMEDIATE EXPERIMENTAL FACILITY IS REQUIRED BEFORE A FULL-SCALE FUSION DRIVER IS BUILT

- IT IS NOT REASONABLE TO PROCEED TO A MULTIMEGAJOULE DRIVER IN ONE STEP
- BEAM FOCUSING LOOKS GOOD THEORETICALLY BUT EXPERIMENTS ARE REQUIRED
- BEAM-TARGET COUPLING LOOKS GOOD THEORETICALLY AND EXPERIMENTALLY BUT SKEPTICS REMAIN. MORE EXPERIMENTS ARE REQUIRED

Figure 10

I would say that the program proceeds toward civilian applications in several steps (Fig. 10). One intermediate step is explained on the next slide. It is something we have called a high-temperature experiment where we would hope to prove to people that the coupling of ions to the target is classical, that the beam transport can be handled, and that this can be done at a level which produces a significant heating of the target. If completed successfully, one will have accomplished a major step in the extrapolation to a full megajoule driver.

ICF WITH LOW GAIN TARGETS CAN PROVIDE ECONOMIC PRODUCTION OF TRITIUM

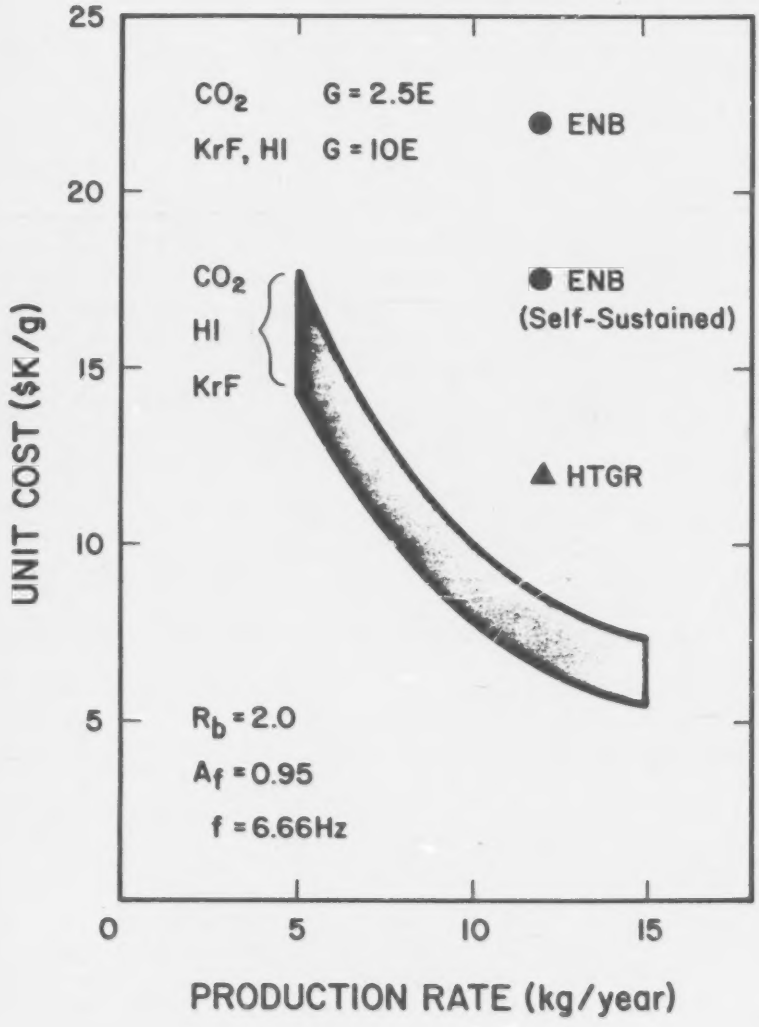

Figure 11

IF-VG-282

The last Viewgraph (Fig. 11) addresses the ability to produce tritium, which any fusion program must be able to do. We have done a series of studies on the economics of tritium breeding using various ICF drivers with the assumption that the target can achieve at least a gain of 10. As shown by the equation on the left, the gain is assumed to be linearly proportional to the energy of the driver where that energy is in megajoules. So, for example, a CO_2 laser or 1 megajoule would be able to produce a gain in target of 2½. This is an assumption. This must be proven.

Gains with heavy ions are a little bit higher because of favorable target coupling. We have used the results of an EPRI study on the cost scaling of the particular drivers shown. For cost of reactors we used the results of the work at the particular choice shown, and assuming a breeding ratio of two, one finds an economy of scale setting in at a production rate of approximately 5 to 7 kilograms per year. At this rate the price is competitive with other proposed approaches to tritium production such as electronuclear breeding or high temperature gas reactors. This has not been proven, of course. I only assert that if the target gain behaves as predicted here, then economically this looks like a feasible way to go into tritium production.

Let me just summarize, then, by going back over some main points. I think ICF should be considered as a part of the energy options, I look forward to its transition, particularly for the heavy ion driver, into OER jurisdiction, because we do know that on some scale it works. We have predictions now for target performance that we believe are more valid than those used in the beginning of ICF, and these predictions indicate we can achieve adequate gains from the target. By using an accelerator you realize many of the attributes you would like in a civilian application. Principally, by including this ICF option one has opened for himself alternative avenues to the problems of engineering fusion for the long term, long-life civilian application.

With that I close. Thank you for your endurance to this late hour.

Mr. RICHARDSON. Thank you very much, Steve. That is a fascinating account of the physics and engineering involved. Let's take a few questions now. Let's leave time for Mel Gottlieb to have a few concluding remarks. Then I am informed that around five o'clock Congresswoman Bouquard will come personally to express her thanks to you for participation in this workshop and to wish you and the program well.

Any questions that you would like to direct to Steve or to Dick?

Dr. WATSON. You say we really aren't addressing any of the energy issues, or only minimally. What is it we would be doing to address energy issues? Reactor studies perhaps?

Dr. SCHRIEVER. Let me rephrase that comment a little bit. We are not doing anything that is solely related to commercial energy production. Examples of such experiments would investigate low cost, rapid repetition rate, high availability drivers, or heavy ions, to a point that they could be put into a commercial reactor application.

Reactor materials, reactor design, power plant design, there are the kinds of things that you need to do only if you are going to

build a power reactor, we are not providing resources for such work at this time. Many of the things that we are doing, are of course, just as fundamental to the energy problem as they are to the physics problems we are interested in. We have to learn to make a fusion pellet burn before we can have fusion. That is the direction in which our primary activities are oriented. But it is in the drivers, in the engineering designs and engineering materials, and in the system studies, that we are putting only a minimal, almost token amount of resources at the present time. The heavy ions that Dr. Rockwood mentioned is a perfect example of an area where we would see the need for funds to go to keep moving on an energy track.

We have made arrangements to transfer that activity over into the groups in Energy Research that are knowledgeable about accelerators.

Mr. EVANS. I would be interested in commenting on the extent of international cooperation now, what you would foresee as ever being possible or desirable.

Dr. SCHRIEVER. At the present time we do not encourage international cooperation in inertial fusion. That stems from the sensitivities associated with the technology as they relate to proliferation. We do have some activities going on with other countries relevant to laser technology, but not to fusion, per se. I think we anticipate that at sometime in the future we may be able to expand this in some controlled ways but I don't see that clearly at the present time.

There is a great deal of activity going on in other countries—good work. And I think we can make very good technical arguments for the values of such cooperation.

Dr. ROCKWOOD. I would add to that. The Japanese program particularly is very active on a broad front at the research level as the gentleman said earlier today. They are covering at least as many aspects of the problem as the U.S. covers.

Dr. BANGERTER. It is also true there is an effort in England and Germany that is comparable. There has been a fair amount of cooperation. At least good cooperation.

Mr. RICHARDSON. Let me ask one last question. It is going to be a very simple-minded one. If you put one megajoule into the pellets now, how much energy are you getting out?

Dr. SCHRIEVER. If we had a megajoule to put into a pellet at this point—

Mr. RICHARDSON. I thought you said you have a megajoule.

Dr. SCHRIEVER. Our large laser facilities would be like a tenth of a megajoule. Our larger pulse power facilities would be like a megajoule. Under the optimal kind of configuration, a megajoule driver ought to yield something between unity and a factor of a few, perhaps as many as 10. Perhaps Dr. Bangerter would comment on that.

Dr. BANGERTER. I don't know. Until you have done it you don't really know. In fact, a megajoule, yes, somewhere between 1 and 100. In a megajoule, that may have been a poor choice. That is where there are very large uncertainties. We think as soon as one gets up to the 3 or 4 megajoule level, the physics certainties are much smaller and predictions are smaller.

Mr. RICHARDSON. Let's now turn to the last part of the program. It is fitting that the closing remarks will be given by Mel Gottlieb, former director of the Plasma Physics Laboratory at Princeton, a man who has devoted his entire scientific career to this question and a man who has also given very good service to the National Academay of Sciences as a member of the Magnetic Fusion Committee.

STATEMENT OF DR. MELVIN GOTTLIEB, PRINCETON PLASMA PHYSICS LABORATORY

Dr. GOTTLIEB. It is a great pleasure to be here.

I have been at the IAEA meeting for this past week. We have been going over the achievements and objectives of the program. It is obvious there are three major thrusts at present. The first thrust you have heard a lot about is attainment of zero energy output so to speak, an energy gain of one. That thrust as you heard before is taking place in four different nations, or groups of nations, the United States, the European Economic Community, Japan, and the Soviet Union, with first operation of these devices anywhere from 3 months to 3 years off in the different cases.

Two of them will produce conditions in a hydrogen plasma such that if a D-T mixture were submitted for the hydrogen then you would get energy balance. But obviously you won't get any energy production if only hydrogen is used. Similarly, the mirror program is engaged in a larger effort at Livermore to produce an equivalent gain of one in the hydrogen plasma in the tandem mirror device. So that is one major thrust in the program to actually achieve energy balance conditions.

The second major thrust is toward increased effectiveness; that is, one would like to, have the most attractive form of a fusion system, whatever that may be. I will speak only about the magnetic confinement program.

As examples of this effort within the Tokamak program we are working on systems to improve the heating methods. Ohmic heating is used for the initial phase, but this must be followed by a second phase of heating which up until recently has been mainly in the form of energetic neutral beams. These are not ideal for reactors for several reasons. One is that as the devices become larger and larger, the injection energy has to increase which tends to make the process more difficult. Present injection systems start out by accelerating and then neutralizing positive ions. But as the injection energy increases, the efficiency of these positive ion based systems falls rapidly. Negative ion based systems would avoid this problem, but they are not now available.

The second difficulty with neutral beam systems is that they have to be located close to the Tokamak. This is a real disadvantage. It would be better to have the power source located far enough from the reactor to allow hands-on maintanance. Radio frequency heating would avoid these problems. The power source can be in another building. The energy transmission can be via curved wave guides so that the neutrons produced in the reactor can be shielded from the RF power source.

The third attractive aspect of RF energy is that RF waves can be used to drive a current in the Tokamak.

The one very significant demonstration of this process has taken place in the last year. RF driven currents up to several hundred kiloamps have recently been achieved. Thus creates the possibility

that a Tokamak will not have to be pulsed but can run at steady state, thereby avoiding many engineering problems associated with fatigue.

This is one example of an attempt to make a Tokamak a more effective device; one that is producing favorable results.

By the way, this has all happened very recently. Two years ago at the previous IAEA meeting, there were results reported showing that RF currents could actually produce small currents—really a demonstration of the principle. But now we deal with megawatts of power: hundreds of kiloamperes of current.

There has been similar progress in the mirror devices. In the case of mirrors of about 5 years ago, it looked as though it would be difficult to get much energy gain. Gains of 1, 2, or 3 were spoken about which required such large amounts of circulating power in a mirror reactor as to make unattractive.

Now, the tandem mirror principle has been demonstrated. The thermal barrier which in principle should strongly enhance energy containment is about to be under study. With such systems, it looks possible to attain energy gains of 10 or better. That has converted the mirror from a very unlikely source to an interesting competitor with Tokamak.

In the search for increased effectiveness, other basic confinement schemes are being studied. They are generally more than a decade behind in state of development.

But some of them do provide at least a hope that one could build systems that are significantly simplified particularly with respect to the coils.

So all these I put under the category of increasing effectiveness.

The third major thrust is that of achieving of a better understanding. Here devices of all sizes are involved. We would like to understand the limitations in the present experiments. For example, have we seen the limit in the pressure that a given magnetic pressure can confine in a given device? If we double the pressure that can be held by a given field, the power output would go up by a factor of four.

So it obviously very important to get the betas high enough—betas representing particle pressure compared with the magnetic pressure—and at this meeting betas were reported that are at least sufficient for a reactor, although one would like them still higher.

This demonstration was performed at magnetic fields substantially smaller than projected reactor levels. It is reasonable to anticipate that similar beta values would be allowed at the high magnetic field level.

We want to understand the limitation in the present experiments, we want to know better the scaling that is involved in projecting to actual reactor conditions.

In the past, we had been operating with ohmic heating and in that case, we had attained a good description of containment scaling. It is clear now that the scaling changes when other methods of heating are applied such as neutral beam heating and RF heating. There is not as yet a single explanation for what is seen.

The containment looks adequate, but it is different. Containment scaling under these conditions must be better understood before a minimum reactor size can be specified.

So those are the three major thrusts not only of the U.S. program, but the world program as well.

A minor thrust has been engineering. We certainly expect that the amount of engineering work required in the future is going to be in all probability larger in magnitude than that has been involved in the study of the basic physics of plasmas. We would all like to get at these design problems, and development problems.

But we have to operate within budgetary constraints. I think that the Magnetic Fusion Engineering Act of 1980 envisaged a very substantial increase in the effort to be devoted to those engineering tasks because they had a particular time scale in mind.

With the present budget levels, the Office of Fusion Energy has decided that instead of a broadside approach to these engineering problems, they would study instead problems called critical problems, that is, those which are the most critical to the attainment of fusion, and concentrate the limited amount of funds available on those particular critical issues. I think this is a sensible approach.

I would emphasize that what you do depends on the time scale. If you decide when you want to achieve the objectives of fusion energy, you can optimize the program to that objective.

I am sure most of you noticed the fact that the long-scale programs of both the European Economic Community and of the Japanese look almost identical with the program that was set up to meet the objectives of the Fusion Engineering Act of 1980.

Now, part of that, of course, is the inevitable result one gets when one studies the results of another group, there is a tendency to follow the lead. But certainly these are independent groups, and in going over our plans they were led to the same conclusion.

Even though we were the first to develop a plan along those lines, a comparison of trends among the three groups indicate the possibility that the United States might be the last to achieve them.

Certainly the energy needs in this country are not the same as the energy needs in Japan. Japan has a much more acute problem than we.

One prospective solution is magnetic fusion, inertial fusion is showing promise, too. They are now much closer to their goals. In the first IAEA meeting I attended in terms of confinement time, multiplied by temperature, and by density, we were about a factor about a hundred thousand below where we are now. Now we have only about a factor of between 10 and 100 to go.

Finally again I would repeat my comment of a little while ago on the international cooperation and industrial participation. There are tensions between these two. I don't think these tensions are there when you are at the research stage. So I think with respect to alternate concepts, for example, there is no reason in the world that I can think of why we should not have a very active international cooperation. A lot more can be accomplished within existing budgetary constraints.

It is when you get close to commercial application that the tensions start to appear because industry is very concerned and justifiably so about proprietary aspects one could expect, that as you approach commercialization, international cooperation will substantially decrease.

So I think we can have international cooperation, we should have it, and if we do it in a controlled and sensible way, I think we will all gain.

With respect to industrial participation, it is also clear this has been an objective of ours for a long time. We at Princeton talked for many years that we would have acoomplished our job when we didn't have it any more, when it was over in industry. However, there is a natural role for industrial participation which changes as a program progresses from basic research to end product manufacturing.

When industry participates, it should be on a continuing basis. That is, giving a role to industry for a couple of years and then removing it certainly does not help industry and does not help the laboratories and does not help the Government. You have to pay a price for training industry people. If the training is simply lost at the end of that individual activity you really haven't gained much.

Industry is playing an important role now and will continue to in terms of design assistance, in terms of A/E activities, in terms of fabrication and so on. This role will grow as we get closer to a reactor.

Mrs. BOUQUARD. Thank you very much, Dr. Gottleib. It is certainly very good to be with all of you here. I am sorry I could not attend today's session earlier, but I look forward to reviewing your testimony today. I plan to make use of this as part of our background and materials from which we will try to develop our programs and establish our priorities for next year.

I do want to commend you for your very excellent workshop that you have had today. Your input is very essential to our program.

Establishing priorities within the budgetary constraints that we are facing will be even a greater challenge in the coming year than we had to face this year. I hope as we do move into the next budget period that we can have a sense of stability and a sense of direction in our programs and in our priorities so that we don't have this on-again, off-again funding scenario that we have had for the past 6 years.

Such funding uncertainties are detrimental to the private sector, to our affiliations with our allies and to everyone involved.

I would like additional input from you gentlemen, if you have anything else to add.

Mr. RICHARDSON. Mrs. Bouquard, we certainly have had a full day and a busy day. We have heard quite a spectrum of material, all the way from DOE through some discussion of industry participation, the program including alternative approaches to fusion. I know that the panelists appreciate you coming and giving us a concluding personal word. And we feel a good day's work has been done.

We want to thank you for the opportunity to come together.

Mrs. BOUQUARD. I certainly appreciate your being here.

Thank you.

Mr. RICHARDSON. There being no further questions and no last words, these proceedings are adjourned.

[Whereupon, at 5:17 p.m., the workshop was adjourned, subject to the call of the Chair.]

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